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To the Editors of the Medical and Phyfical Journal.

GENTLEMEN,

AS an extraordinary fact, perhaps you may be induced to give a place in your Journal to the following morbid diffection; the drawing is a correct reprefentation of the difeafe, taken by myfelf from the preparation. Inftances have occurred of hernia through the diaphragm in the human fubject, but thefe inftances are extremely rare; and when they have been difcovered, a mal-conformation of the parts has at the fame time been detected, which lufus naturæ was probably, in moft cafes, the primary caufe of the accident. In the prefent inftance, it did not appear upon diffection, that the diaphragm was malformed, or any of the adjacent parts; but the hernia appeared to have been produced by the common caufes of hernia. As the erect pofition of the human body is peculiarly favourable to the production of the different fpecies of hernia which pafs through the ring of the external oblique mufcle of the abdomen, and underneath that portion of its tendon, denominated the ligamentum poupartii, fo likewife is the horizontal pofture of different quadrupeds particularly favourable to the production of hernia of the diaphragm; but it does not appear that this morbid derangement has been detected by anatomifts in the brute creation, at leaft, I am not acquainted with any one inftance upon record; that it fhould not very often happen in the human fubject, is not to be wondered at; indeed, fo peculiar is the fituation of the diaphragm, and fo unfavourable is the erect pofition of the human body to its production, that, upon a curfory view of the fubject, we might, with no fmall fhare of plaufibility, as well as probability, on our fide, pronounce that it never could happen, unlefs there was fome very extraordinary mal-formation of the parts immediately concerned.

The drawing which I have now ſent you, has been ſhewn to ſeveral medical men, who have thought it worthy to be preſerved. If, Gentlemen, you ſhould be of a ſimilar opinion, it is much at your ſervice. I am, &c.

JAMES HUME SPRY,
Member of the Royal College of Surgeons, London.

Alderſgate Street, December 30, 1800.

The animal was obſerved to be in very great pain, and was ſeized ſuddenly, without having ſuffered any previous injury from external violence. It cried frequently, with a peculiar voice not to be deſcribed; and during reſpiration, (which was extremely difficult) it made a noiſe, which appeared to me, ſimilar to that which a child makes who labours under cynanche trachealis. Soon after its firſt ſeizure, it vomited very much; and upon attempting to put a little liquid into its mouth, the animal's tortures ſeemed to be very much increaſed: From the time of its ſeizure to its death, it was about two hours and a half, or three hours.

The ſtrangeneſs of the animal's complaint, induced me to diſſect it; and the following is a deſcription of the appearances which refer to the drawing, by which it will be much better underſtood. It is to be obſerved, that all the parts which were not abſolutely neceſſary to explain the diſeaſe, were removed, and omitted in the drawing.

AAAA. The Lungs.

B. The heart on the right ſide: the pericardium was ruptured, and the heart was forced from its natural ſituation by the ſtomach, which was exceedingly diſtended with air.

C. The Stomach; this viſcus occupied the major part of the thorax; and the lungs were forced up to the ſuperior part of the cheſt, and compreſſed in every direction by it.

DD. The diaphrgam.

E. The Duodenum.

F. A maſs of omentum, with the pancreas and ſpleen paſſing with the duodenum through the tendinous opening of the diaphragm.

GG. Renes.

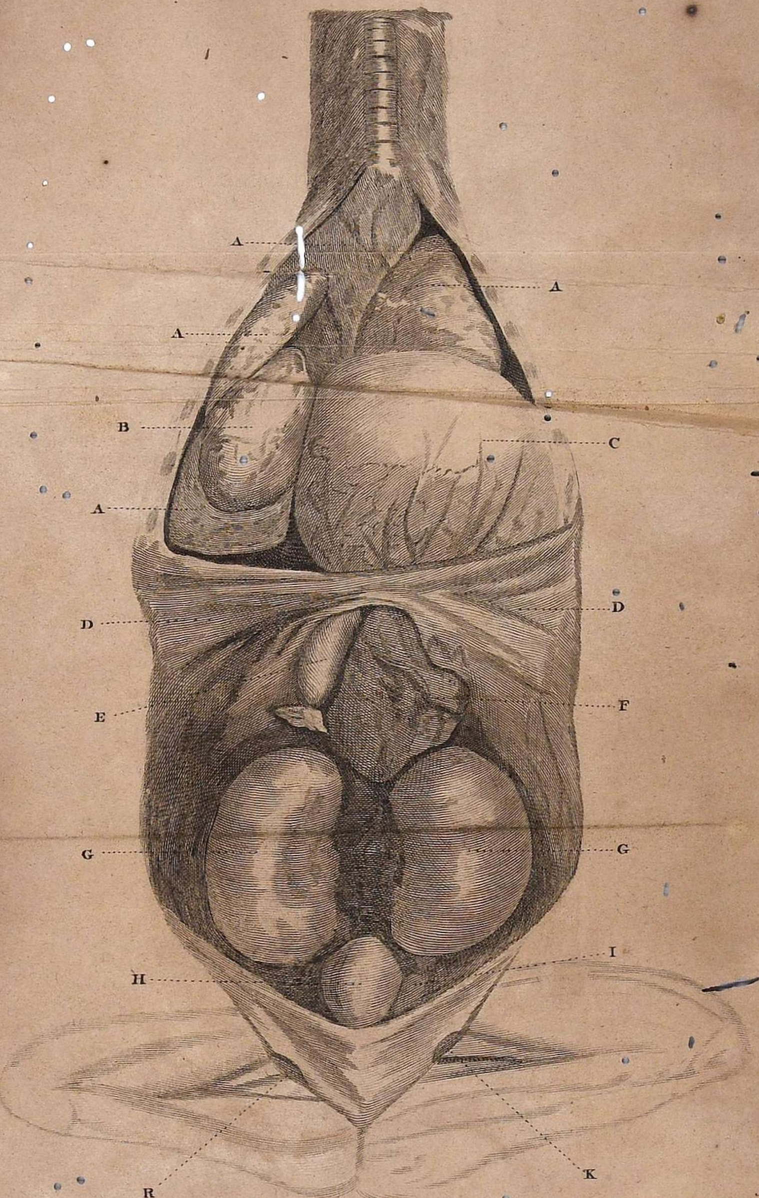
H. Veſica urinaria.

I. Inteſtinum rectum.

KK. Opening of the external oblique muſcle of the abdomen, commonly called the abdominal ring.

To

Hernia of the Stomach through the tendinous orifice of the Diaphragm in the Felis Catus domesticus



Spry del. Skelton sculp.

Medical Journal No. 24.

Printed for R. Phillips, No. 71 St. Pauls Church Yard.

To the Editors of the Medical and Physical Journal.

Gentlemen,

I Hope you will excuse the liberty I am going to take, by pointing out to you what appears to me an error in your truly valuable Miscellany; as scientific men, I think you will readily excuse it, and if I am not very much mistaken, will coincide with me in thinking, that the mode of arrangement adopted by some of your correspondents, is not only irregular, but unworthy of learned men. I allude to the arrangement of diseases in the different districts in London. What can be more discordant to the eye, or to the ideas of a man who admires his profession, and is a lover of science, and who is acquainted with the great accuracy of the celebrated Dr. Cullen in his Synopsis Nosologiæ Methodicæ, as well as the Nosologists prior to his time, and particularly Sauvages, than to see a catalogue of diseases put together without method, with half Latin and the other half English names? Surely, it would be as easy to adopt technical terms entirely, or quite reject them in favour of our native language. I confess that, in my opinion, technical terms ought solely to be used; and I am very certain that if the method of arrangement was more systematic, it would conduce much to the knowledge of disease, independant of shewing the number and quality of diseases, which happen within certain stated periods, in any particular district of the metropolis, or any other place. Suppose, for instance, Dr. Cullen's method of classification was chosen, each disease ought to be placed on the list as it stands in his catalogue; and then the medical student, or the juniors in the profession, would see at one view the nature of each disease, and the affinity they bear to each other.

Classis.	*Ordo.*	*No.*	*Genus.*	*Species.*	*Varietas.*	*No.*
1 Pyrexia	1 Febres	1	Tertiana			
		4	Synocha			
		5	Typhus	Petechialis		
	2 Phlegmasiæ	10	Cynanche	Tonsillaris		
			Ditto	Maligna		
	3 Exanthemata	26	Variola	Discreta		
	4 Hæmorrhagia	37	Hæmoptysis	Phthisica		
2 Neuroses	1 Comata	42	Apoplexia	Sanguinea		
3 Cachexiæ	2 Intumescentiæ	75	Anasarca	Debilium		
	3 Impetegines	85	Syphilis			
4 Locales	1 Dysæsthesiæ	97	Paracusis			
	3 Dyscinesiæ	112	Paraphonia	Rauca		
	5 Epischeses	127	Amenorrhœa	Emansionis		
	8 Dialyses	149	Psora			

 The

The above table will illuſtrate my meaning. The utility of ſuch a table muſt be evident to every thinking perſon; to the ſtudent it would be very valuable, as it would enable him to ſee at one view the nature of any diſeaſe; and his mind would be more familiar with the terms of his profeſſion; not to mention, that it would make him ſtudy the works of the immortal Cullen, and thoſe of other noſologiſts, at preſent too much neglected. When I firſt commenced the ſtudy of medicine, I found a table of this kind of the greateſt uſe to me; it very much aſſiſted me in acquiring a proper knowledge of Dr. Cullen's Noſologia Methodica; the terms became familiar to me, and when I was at a loſs for the definition of any particular term, by looking at my table I immediately ſaw the claſs and order, as well as the genus, and from the number of the genus which was prefixed to each, I made the reference to the Noſology for an explanation.

Thus, gentlemen, I have freely delivered my ſentiments upon a ſubject, which I think requires amendment; and if you think the above remarks may be of any ſervice to our junior brethren, you are at liberty to inſert this paper in your Journal. I am, &c.

JAMES HUME SPRY.

To Dr. BATTY.

DEAR SIR,

THE encloſed letter from Dr. Harriſon appears to me intereſting to the public, and particularly ſo to thoſe who are friends to the Vaccine Inoculation; if you think it proper for your publication it will gratify me to ſee it there, and I am ſure it will alſo be agreeable to our friend Dr. Harriſon. I am,

DEAR SIR,

With real eſteem and regard, your very humble ſervant,

Soho Square, Jan. 13, 1801. J. BANKS.

To the Right. Hon. Sir J. BANKS, K. B.

SIR,

I received a letter laſt night from a correſpondent in London, who informs me that you had expreſſed a deſire to be made acquainted with the particulars of a caſe of Cow-pox Inoculation, which occurred in this neighbourhood, and ſix months afterwards was followed by the Small-pox.

Upwards

Upwards of twelve months since, the nurse-maid, and two children of my friend, the Rev. Marmaduke Allington, of Swinop-House, were inoculated for the Cow-pox, with matter which had been sent from a great distance. Neither of the infants were infected, but the maid had considerable inflammation on the arm, and although it was not attended with indisposition, she remains insensible to the variolous impression. From the nurse were inoculated a female servant, a third child, and Miss Fanny Allington, who was then about five months old, had the operation repeated. The servant did not take the Cow-pox, but the children were supposed at the time to pass through it in an easy manner. Though I did not see any of the patients before they were inoculated for the Small-pox, I had frequent conversations upon the subject with the attending surgeon, Mr. Sexty, who was always doubtful and dissatisfied with the experiments. From him, and some other respectable friends, who have carefully enquired into the phenomena, I am informed, that in Fanny's arm the progress of the pustule was unusually rapid, and was characterized by an areola that was neither extensive nor circumscribed. Mr. Sexty is an accurate and respectable practitioner, but, on account of other avocations and his distant residence, the patients were only visited occasionally; and as there was no constitutional indisposition, Mr. Allington paid them no particular attention. From these causes the appearances on the incised parts cannot be minutely detailed, but I think a sufficient number of circumstances may be collected, to enable us to come to a satisfactory conclusion on the subject. In both children the incised parts were inflamed, and a fluid was produced, but they had neither eruption nor illness. With matter taken from the arm of Fanny, the maid was again exposed to the vaccine disorder. From her several others were infected, and when the Cow-pox had been propagated through different subjects, Mr. Allington's other child was likewise inoculated. Six months afterwards they were all exposed to variolous inoculation, and Fanny took the disorder. She had a mild Small-pox with a moderate eruption. Hence it appears, that Fanny communicated a security against the Small-pox to others, although she herself remained liable to its influence.

Several well attested instances are upon record, where variolous contagion from inoculated pustules has produced a perfect Small-pox, and yet the subjects from whose arms it was taken had only a local infection, and afterwards passed through the Small-pox, either in the natural way or by inoculation. If it were necessary, I could corroborate this fact by detailing several cases, which either occurred in my own practice, or have been

been communicated to me by gentlemen of veracity and judgment.

In the present instance I cannot undertake to determine, whether the failure was owing to a *peculiarity in the child's constitution, or to the matter with which she was inoculated.* When the variolous, or vaccine, contagions are carelessly dried and preserved, or are taken from old pustules, the nature of the poison is sometimes so much changed, that it neither produces its peculiar disease, nor does it afford any real security. In other cases, where a variolous pustule arises upon the inoculated part, and passes through its different stages without producing any general infection, the failure is not to be imputed to the matter employed. In such instances it must have been occasioned by some unsusceptibility, or unfitness to receive the peculiar impression. Nurses are liable to variolous eruptions, from attending upon children in that complaint. With them they are purely local; and when persons who have never had the Small-pox or vaccine disorders, remain proof against infection, it arises from some constitutional peculiarity, which takes place at the time, and with which we are just as much acquainted as with the change that is produced by the action of variolous or vaccine matter upon the general habit. Sometimes, though rarely, this disposition continues through life; at other times it affords only a temporary protection; and it is worthy of remark, that cases have occurred where the same person has been twice affected with the Small-pox.

Experienced practitioners are enabled to determine with confidence, whether the patient will continue to be exempt from the Vaccine and Small-pox Diseases, by observing the appearances that take place in the inoculated arm. When a fluid is produced too soon after inoculation; when the pimple is too much elevated, has no areola, or only a small and irregularly inflamed margin; or when it is accompanied by considerable efflorescence, and forms a scab before the usual time, there is reason to conclude the person has only gone through the local complaint, and has received no protection against the consequences of any future exposures.

Whether the failure in this case is to be imputed to the matter employed, or to the child's habit of body, it affords no solid objection to Vaccine Inoculation, since the same want of success has occurred in the Small-pox. It tends, however, to confirm the close analogy that has been found to subsist between these two diseases; and although, in the infancy of enquiry, it may be better to record facts than indulge in speculations, I am inclined to believe, the more we become acquainted with

their

their laws, the greater will be found the refemblance between them.

I have the honour to be, with great refpect and efteem,

Sir,

Your much obliged,

And faithful humble fervant,

Horncaftle, Dec. 30, 1800. E. HARRISON.

P. S. About two years fince we inoculated nearly one hundred perfons for the Cow-pox, with matter which you had obligingly provided for the Public Difpenfary. In one inftance, a fmall tuft of hair, refembling that on the child's head, was obferved to grow from the incifed part immediately after the defquammation.

To the Editors of the Medical and Phyfical Journal.

Gentlemen,

AMONGST the many difcoveries that have been made for the advantage of the public, none furely ever claimed greater attention and gratitude than that of the Cow-pox.

I mean not to enter into any medical difcuffions of this difeafe, but only to ftate a few plain matters of fact, which have paffed through my own hands or under my obfervation.

Having procured the genuine Vaccine matter, I firft inoculated my fervants; one of whom expreffing a wifh to know, by experiment, if it were an effectual preventive of the Smallpox, I permitted him to expofe himfelf to its contagion, which he did, by lying with and inhaling the breath of a boy who was blind, and fuppofed to be in the agonies of death, with this cruel diftemper. From this enterprife my fervant felt no ill effect whatever. The mother of the boy, who was ill with the Small-pox, fhortly afterwards fickened with the fame diftemper, and foon became a deplorable object in it; when her hufband, who dreaded the malady, implored me to inoculate him with the Cow-pox; which I did, and he became an attendant on his wife and fon, in perfect fecurity: But be it underftood, that till he had been inoculated for the Vaccine difeafe, he kept aloof from his family.

I have likewife inoculated others for the Cow-pox in a neighbouring town, whilft there was in it, at the fame time, a general inoculation for the Small-pox; when not one of my patients

patients felt any effects from the latter, though in a conſtant communication with their infected neighbours.

Never have greater opportunities or more inſtances occurred, to manifeſt the certain and bleſſed effects of the Cow-pox, than in this pariſh and neighbourhood; for when I made my firſt experiments, the Small-pox had entered the village in a dreadful form, ravaging and making devaſtation in every quarter: but every one who had received the Vaccine inoculation, eſcaped unhurt. Whilſt a huſband has died by the former, a wife has been ſaved by the latter. And amongſt the many hundreds that have been inoculated in the neighbourhood with the genuine Vaccine matter which I introduced, not one inſtance has occurred in which it has failed of its beneficent and ſalutary effects.

I hope ſoon to hear, through the medium of your uſeful Journal, that the indefatigable and ingenious Diſcoverer has felt his Country's gratitude for his labours, as they could not have been carried to their preſent perfection without infinite pains and great expence: Firſt, to withſtand the medical cabals that generally attend ſuch diſcoveries; and, ſecondly, to render it ſuperior to the power of Envy and Detraction to bury in obſcurity its glorious and wonderful effects.

May not only England then, but all Europe, which now receive the benefit of it, unite to expreſs, by every poſſible means, their gratitude to the diſcoverer of ſuch a providential method of chacing from the earth one of its bittereſt maladies. I am, &c.

J. S. GALE.

East Woodhay, Hants, Jan. 9, 1801.

To the Editors of the Medical and Phyſical Journal.

GENTLEMEN,

AS I am very certain the contents of the following letter from Mr. W. Sharp, who is now retired from buſineſs, on the ſubject of the Cow-pox and its origin, is intereſting, I wiſh much to have it inſerted in the Medical and Phyſical Journal. I am, &c.

Tusmore, Jan. 16, 1801. W. FERMOR.

" Dear Sir,

" WHEN I had the pleaſure of meeting you the other morning, you aſked me, if the Cow-pox prevailed amongſt the cattle

tle in the North of England? I could not then anſwer your queſtion; but I enquired, and was informed by a letter this morning from Durham, that the diſeaſe is not known there among the cattle; and alſo, that they had heard of it in four counties only, namely, Glouceſter, Oxford, Bucks, and Northamptonſhire.*

"From circumſtances which have fallen in my way, I believe that the diſorder ariſes from infection brought to the cows by hands employed about the diſeaſed heels of horſes; and in conſequence, that it could not be known there, as the cows are univerſally milked by women, who have nothing to do in the ſtable.

"Wiſhing all poſſible ſucceſs to the Inoculation of the Cow-pox, which you have ſo meritorious a ſhare in promoting, I am,

"Dear Sir,

"Your's, &c.

"Wm. SHARP."

Wicken Park, Bucks,
Dec. 18, 1800

A Case of strangulated Crural Hernia, where the Operation succeeded after the Obstruction had continued Six Days.

By G. Borrett, Surgeon, Great Yarmouth, and Member of the Royal College of Surgeons, London.

To the Editors of the Medical and Phyſical Journal.

Gentlemen,

On Tueſday, Sept. 25, 1800, I was requeſted to attend Mrs. Warner, of this place, in conſultation with four medical gentlemen. We found her with a *Crural Hernia* incarcerated, vomiting a quantity of fæcal matter, with hiccough, cold extremities, and a quick, ſmall, but irregular pulſe.

The inteſtine had already been ſtrangulated five days, during which time the uſual treatment recommended for reduction was ſteadily but unſucceſsfully purſued. The tumour was about the ſize of a pullet's egg; and in conſequence of its not being tender to the touch, reduction was attempted, but without ſuc-

* It has been obſerved alſo in Somerſetſhire, Devonſhire, Wiltſhire, Berks, and ſome others.

cefs. The operation appeared the only chance to fave her life, which was urged to her, but fhe would not confent. The cold applications were continued, pills compofed of Ext. Colocynth. c. and Calomel and a grain of Opium for a dofe, were prefcribed, and enemas of tobacco. At ten o'clock the following morning we vifited our patient. The enemas had acted powerfully on the fyftem, but produced no evacuation by ftool; the vomiting of fæces continued. Being apprehenfive that fhe could live very few hours, fhe begged the operation might be performed, to which we agreed, as the only means of faving her life; although we now much doubted its fuccefs, as her pulfe was become very feeble, and fhe had the *facies hippocratica* ftrongly marked.

Every neceffary preparation being made, I proceeded to the operation; expofed the fascia, ligamentum Poupartii, the hernial fac, and its contents; I then divided the ligamentum Poupartii, and afterwards, with fome difficulty, paffed Pott's Biftory, and divided the ftricture (which was confiderable) at the mouth of the fac. The rupture was Enterocele, and the portion of inteftine included, which was the fize of a nutmeg, appeared of a livid colour: Preffure upon the inteftine gave pain and excited retchings, which encouraged me to hope it had ftill fufficient life to have its action reproduced.

The fac had defcended for fourteen years, and more than once fuffered inflammation; it was thickened in confequence, and refembled a cod's found, and completely adhered to the furrounding adipofe membrane; a great portion of it was diffected out. The wound was drawn together by futures; light dreffings were applied, and fecured by a T bandage. Small opiates were ordered every four hours, to allay the irritability of the ftomach, and fupport her ftrength; and broth clyfters, after fome days, reftored the natural periftaltic motion of the inteftines. Mrs. Warner was the mother of thirteen children, and at this time in the third month of geftation; fhe did not mifcarry, and is now quite well: She wears a trufs. This cafe, and many others recorded, fhew, that the operation for Hernia may fucceed at almoft any period of ftrangulation, and encourage us never to defpair of our patient.

I am induced to communicate the above cafe, becaufe Mr. Geoghegan, (No. xx. of your ufeful Medical Journal for October) in his "Strictures on the ufual Practice in ftrangulated Hernia," reprefents the danger of the operation, abftractedly confidered, to be imminent, and appears an advocate for unlimited procraftination; from which, in my eftimation, the danger wholly arifes, and becaufe I know of no clear account of the operation for *Crural Hernia.*

Whoever

Whoever will conſult Pott on Ruptures, will ſee the moſt judicious treatment and decided reaſons for his mode of conduct, reſpecting the operation. "There is no circumſtance," ſays he, "attending ruptures with ſtricture, in which more variety is found than in the time which they will ſafely admit to be ſpent in their reduction; ſome have been ſucceſsfully replaced at the end of eight or ten days, others have proved fatal in one." The attention of the medical world ought, therefore, to be directed to the *time* when the operation, in different caſes of ſtrangulated Hernia, ſhould be performed; from delay every thing is to be dreaded; and many valuable lives are daily loſt, that would indiſputably be ſaved. The Surgeon who has a caſe of ſtrangulated Hernia, ought not to waſte the only moments which might ſecure his patient's life by a *well-timed operation* in applying fomentations and placebos. Pott, p. 82 and 83, vol. ii. "There is not, in the practice of Surgery, a point which requires more judgment, firmneſs or delicacy, than to determine the preciſe time beyond which this operation ſhould not be deferred, and for a ſurgeon to conduct himſelf ſo as to induce a patient to ſubmit to it early enough for his preſervation. The two principal circumſtances which have moſt contributed to the infrequency of performing this operation are, a dread of great hazard from the operation itſelf, conſidered abſtractedly, and a fear of bringing a diſgrace upon it by having performed it too late; *ne occidisse, nisi servasset videretur.**— The firſt then is vaſtly greater than it ought to be, and is moſt frequently the cauſe of the latter; ſo that if the one can juſtly be leſſened, the other will not be likely to happen." Few experienced men then can doubt the propriety of operating early in ſtrangulated Hernia: After the moſt approved treatment has been regularly and conſiſtently tried without effect, and a judicious application of the taxis has been unſucceſsful, why ſhould the life of the patient be riſqued by farther delay of the operation, in which, comparatively, there is no hazard, if it be judiciouſly performed? I am juſtified in this poſitive aſſertion if any inference may be drawn from ſix caſes, four † of which (beſides the preſent) recovered. Of the other two, one died from the operation being too long delayed; and the failure of the other depended on the ſtricture at the mouth of the ſac not having been completely divided; a point in the operation which requires great care and certainty.

* Celſus.

† From one of theſe patients more than a pound of diſeaſed omentum was cut off, without hæmorrhage or any other diſagreeable ſymptom.

I entirely agree with Mr. Geoghegan in the application of cold to the tumour, by cloths wetted in mixtures generating cold, as well as by expofing the body to cold, where the patient can bear it; but I don't fee how the palms of the hands fhould be applied to the fides of the Hernia denominated by writers Bubonocele, which is the term of the difeafe of which Mr. G. treats: In Scrotal Hernia indeed they may; but while the contents of the rupture are fituated in the groin, it appears impoffible that the palms of the hands fhould be applied in the manner Mr. G. recommends, to expel air. The quotation which he makes from Mr. Pott, relative to grafping the tumour, is not only inaccurate, but the application is erroneous, it being in *Scrotal Hernia* Mr. Pott recommends that the tumour fhould be grafped with one hand, to prevent the teftes from afcending and the inteftine from defcending; while, with the other hand, you endeavour the return of the tumour. Had Mr. G. read a little farther, he would have feen the following. "If the cafe be a Bubonocele, there will be no occafion for endeavouring to grafp the tumour; but by continued moderate preffure on it with the fingers, to endeavour the return of the piece of gut." Neither am I inclined to confider the abdominal ring as a metal ring, which therefore "muft be paffive;" is it not more probable, that the diforder in the inteftine is originally produced by the ftricture made upon it by the borders of the tendinous opening of the abdominal mufcle, and that the gut is in general perfectly found and free from difeafe before it becomes engaged in fuch ftricture, and that the inflammation of the inteftine is the confequence? If the abdominal ring is paffive, how will Mr. G. explain the frequent reduction of ruptures during the relaxation and fyncope occafioned by the tobacco enema, and the ftill more frequent reduction which takes place a few hours before death, from thefe very parts having loft their tone, and no longer making refiftance to the return of the Hernia?*

In the operation for Crural Hernia, the epigaftric artery and fpermatic chord in the male, and ligamentum rotundum in the female, are parts lying in the way of the knife, and particularly increafe the difficulty and hazard of the fuccefs of this operation. Benjamin Bell, in his account of the operation in Femoral Hernia, difcourages us by faying, "Whether the incifion be carried directly upwards, or even obliquely outwards or

* Since the above Cafe, I am informed a female has died in Yarmouth of ftrangulated Crural Hernia, to whom medical affiftance was not called till fhe was in articulo mortis; when upon applying the fingers to the tumour, a crepitus was heard, and the contents went up: The patient died foon after.

or inwards, these parts must generally be divided." Pott is not more clear and determined, and recommends, after the hernial sac, intestine, and ligamentum Poupartii, &c. are exposed, to endeavour the return of the intestine before the division of the ligament. Supposing every medical man perfectly acquainted with the anatomy of the parts concerned in Crural Hernia, I shall proceed directly to a description of the operation.

The patient being placed in a proper situation, the incision is to be begun above the tumour, laying bare the fascia and the sac. You then see the ligamentum Poupartii, binding down the sac, which is to be divided; but as you will have the epigastric artery, and spermatic vessels in man, crossing each other *immediately over* the tumour; and in woman the epigastric artery and ligamentum rotundum, much care must be taken.—The best mode is, to make a small incision between the fibres of the external oblique muscle, about *half an inch* above the ligament, and a director being passed *carefully and immediately under the ligament, and over the artery,* (which adheres to the ligament by cellular substance) to cut into the grove of the director. If, when this is done, you should find the stricture at the mouth of the sac, it must be divided *inwards* to the pubes, inclining Pott's knife rather obliquely downwards. In this mode you will avoid the artery, chord, and round ligament.

I conclude with apologies for taking up so much of your Journal; but if, through your means, the attention of the Medical World should be directed to an early operation in these dangerous cases, we shall be sufficiently rewarded by the many lives which will be saved. I am, &c,

G. BORRETT.

Dec. 26, 1800.

To the Editors of the Medical and Physical Journal.

GENTLEMEN,

THE following communication may probably appear to you worthy of a place in your Journal, as exhibiting a concise view of the state of disease, during the last year, among the poor of our large and populous town. I am, &c.

J. BOSTOCK.

Liverpool, Jan. 19, 1801.

Account

Account of the Diseases admitted into the Liverpool Dispensary, between Jan. 1, 1800, and Jan. 1, 1801.

Fevers	2603	Diarrhœa	723
Diseases of the Eyes	283	Hysteria	99
Sore Throat	221	Amenorrhœa	320
Inflammation of the Chest	78	Convulsions	32
Consumption	90	Insanity	5
Rheumatism	449	Debility	356
Erysipelas	18	Dropsy	107
Small-Pox	176	Scrofula	142
Measles	119	Rickets	4
Scarlet Fever	10	Syphilis	511
Thrush	55	Jaundice	48
Piles	57	Gravel	68
Hæmorrhagies	114	Worms	198
Catarrh	434	Eruptions and Diseases of the Skin	2287
Dysentery	383	Burns and Scalds	116
Head-ach and Vertigo	159	Abscesses, Ulcers, &c.	868
Apoplexy	8	Wounds, Fractures, and Dislocations	1013
Palsy	29	Cancers	19
Stomach Complaints	658	Ruptures	137
Epilepsy	17		
Dyspnœa, Cough	1523		
Chin-Cough	39		
Colic	102	Total	15165
Cholera	487		

Upon comparing the above List with the account of diseases admitted into the Dispensary in previous years, the first circumstance which arrests the attention, is a very large increase in the total amount of the patients. The numbers admitted in the years 1797, 1798, and 1799, are 12,162, 12,108, and 13,052, respectively, which gives an average for the three years of 12,441 per annum. In the year 1800 therefore an increase has taken place of 2,724, which is above one-sixth of the whole number. To account for so great an addition, we observe that the diseases which are classed under the head of Fever, considerably exceed their accustomed proportion; during the three preceding years the average number of Fevers was 1629; which gives an increase, in the year 1800, of 974. The causes of this additional frequency of Fever amongst the poor, may with great probability be referred to the circumstances which have been pointed out by your Correspondents in the metropolis, as producing the same effect in the sphere of their observations; I refer to a deficiency of food, a want of sufficient cloathing, and great dejection and anxiety of mind. The commercial spirit of Liverpool has happily prevented its inhabitants from feeling the distresses of the times in nearly so great

great a degree as they have been experienced in many other places, yet even here the diſtreſs is widely diffuſed, and in many inſtances extreme.

The Small-pox and Meaſles of the year 1800, have been rather below the average of the three preceding ſeaſons.

Under the head of Rheumatiſm, we find in the year 1800, that the number of caſes has been conſiderably leſs than uſual; the average of the three years being 580, whereas the above liſt indicates only 449. This diminution is obviouſly to be attributed to the unuſual warmth and dryneſs of the ſeaſon; to the ſame cauſe we may alſo aſcribe a great diminution of the uſual number of Catarrhal complaints, which are this year little more than one-third of their uſual proportion.

Another important variation in the liſt of diſeaſes for the laſt year, which muſt alſo be attributed to the peculiarity of the ſeaſon, is the unuſual frequency of the diſeaſes of the inteſtinal canal. During this period, 1593 caſes appear upon the books of the Diſpenſary, under the heads of Dyſentery, Cholera, and Diarrhœa; which is above four times the average number of theſe complaints.

The only diſeaſe which remains to be pointed out, as affording any thing worthy of remark, is that claſs of ailments, which are entitled Debility; theſe, as might be expected, have during the year 1800 almoſt doubled their uſual proportion. They conſiſt in a great meaſure of old and needy females, who, unable to procure a livelihood by their labour, and chiefly dependent upon precarious charity, drag on a miſerable exiſtence, which in the preſent ſeaſon of ſcarcity is rendered doubly wretched.

It may be proper to notice, that Liverpool, beſides an Infirmary, contains a Lunatic Aſylum, where a great part of the caſes of Inſanity which occur among the poor, are admitted; and a Charity for Lying-in Women, where Puerperal Diſeaſes are relieved.

*** *The Editors are much obliged to* Dr. Bostock *for the above Communication; they invite him to continue it annually, and will be glad to receive similar Communications from Hospitals and Dispensaries in every part of the kingdom.*

To the Editors of the Medical and Phyſical Journal.

Gentlemen,

I HATE controverſy, and therefore approve much of the *present* conduct of your correſpondent, Mr. Sheldrake, in dropping the ſubject of diſpute between us. It is certainly the moſt

moſt rational mode of procedure, when he finds his attack no longer *defenſible*, to get rid of it *in the beſt way he can.** Whether his Anſwer, or my Reply, contains moſt *unprovoked*, *illiberal*, and *perſonal abuſe*, I leave to your candid Readers to determine.

I hope, however, you will excuſe my taking notice of a very ſingular paſſage, principally directed againſt me,† in a latter communication from the ſame author. It is as follows.

"Gentlemen, I ſhall now, with your permiſſion, reſume the ſubject I hinted at in my laſt communication, viz. to ſhow, as far as my experience will ſhow, under what circumſtances, and at what periods of life, thoſe diſtortions of the feet, commonly called Club-feet, and more learnedly, *but more vaguely*, *termed* VARUS and VALGUS, may be cured."‡ With regard to the terms *varus* and *valgus*, I may remark, that they have been uſed not only by medical writers, but even by claſſical authors, and in common language, for at leaſt *two thouſand years*, as appears from the following paſſage in Plautus: "*Aut varum aut valgum aut compernem, &c. Mil.* 3, 1, 128; and ſeveral others that I could produce, equally ancient. And during which *long* period they have been *invariably* uſed to ſignify the ſame things; *varus* when the toes are turned in, and *valgus* when they are turned out. It is certainly *one step* towards perfection in any language, when there are *two diſtinct ſpecies* of a thing, to have an appropriate name for each; yet it would appear, that there are ſome people ſo eccentric, or I may rather ſay, abſurd in their notions, as to *prefer one confuſed term, equally inapplicable to either ſpecies, to a proper and diſtinct name for each.* I would aſk Mr. S. what *ſtriking reſemblance* he can perceive between a Club and a Diſtorted Foot, let him take any CLUB he chuſes, from the celebrated Club of Hercules to that of a common *Cow-keeper.* I am, &c.

Paiſley, Jan. 10, 1801. R. WATT.

P. S. In the courſe of a month or two, I ſhall tranſmit you two caſes of *Vari*, which I have now very nearly cured; the one a boy between two and three years of age, the other between four and five. With theſe I ſhall alſo give you a deſcription and drawing of the machine I have uſed for that purpoſe.

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* Medical and Phyſical Journal, Vol. V. p. 9.
† Ibid. Vol. IV. pp. 192, 193. ‡ Ibid. Vol. IV. p. 493.

A Caſe

A Case of Apoplexia Hydrocephalica, with Remarks.

By T. Garnett, M. D.

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ON the 19th of November in the evening, I was requeſted to viſit Miſs H. aged 16, whom I found in a comatoſe ſtate; her breathing was rather ſtrong and unequal, and when rouſed by conſiderable force ſhe did not open her eyes, but put her hand upon her forehead, and cried out in a moſt piteous manner, "O my head!" Her pulſe was 96, and rather full, and the catamenia, which had been regular for near two years, had appeared the preceding week. The patient when in health was of a very lively diſpoſition and remarkably intelligent. About a year before, ſhe had complained for a few days of pain in her head, which was removed by gentle purgatives.

For two days before I viſited her, ſhe had complained of conſiderable pain in the forehead, extending on each ſide to the temples. When I firſt ſaw her, I could with great difficulty get her to ſpeak; but when at length ſhe was ſufficiently rouſed, ſhe ſaid the pain was ſo violent that ſhe thought her ſkull was ſeparating, and immediately relapſed into a ſtate of inſenſibility. The pain had become more violent about two hours before, previous to which ſome ſymptoms of hyſteria had appeared, which were ſucceeded by violent ſickneſs and vomiting.

With a view of alleviating the preſent ſymptoms, I ordered her the following draught:

℞. Tinct. opi gutt. xxv.—Æther. vitriolic ʒj.—Aqu. fontan. ℥ß.—Syr. ſimp. ʒj. M.

And as there was ſome appearance of nervous irritability, I preſcribed the following:

℞. Spir. vol. fœtid.—Tinct. valerianæ vol. Spt. lavend. comp. aa. ʒij.

Of this mixture a tea ſpoonful was directed to be taken every three hours in a little water.

Nov. 20th.—She had paſſed a very reſtleſs night; her ſleep was diſturbed; and at intervals ſhe complained much of pain in her head, ſhooting downwards towards her left ſhoulder. She had been very ſick in the night, and had thrown up the drops twice, but not the draught. Pulſe 100, full, and ſomewhat irregular. She ſeemed always much inclined to ſleep, and when rouſed, almoſt inſtantly cloſed her eyes: She appeared however to have conſiderable averſion to the light, and deſired that the

curtains of the bed might be cloſe drawn, in order to exclude it; notwithſtanding which, the pupils of her eyes, were preternaturally dilated, and ſcarcely contracted on bringing a lighted candle near them. The pain in her head ſhe ſaid was very acute, and never left her for one inſtant. Her ſkin was very hot and dry; and as ſhe had not had an evacuation of the bowels for two days, I directed a ſtimulant glyſter, which brought away ſome hardened fæces.

As I had now reaſon to apprehend that I had to combat that terrible diſeaſe, the Apoplexia Hydrocephalica, I directed the application of a large bliſter between the ſhoulders, and one of the following pills to be taken twice a day:

℞. Calomel.—Pulv. digital. purpur. aa. gr. vj.—Extract. gentian. q. s. ut ft. Pil. vj.

I likewiſe ordered a drachm of *unguent. hydrarg. mit.* to be well rubbed on the nape of the neck and behind the ear, three times a day. I alſo directed the immediate application of three leeches to each temple.

21.—The leeches had drawn a conſiderable quantity of blood, and ſhe thought her head rather eaſier for ſome hours; but the pain was now as bad as ever; the comatoſe diſpoſition had increaſed, and in the night ſhe had been delirious. The pupils of her eyes were much dilated, and did not contract on the approach of a candle. Her pulſe was 76, irregular, and ſomewhat ſtrong. She did not complain of pain any where excepting the head.

22.—Much the ſame as yeſterday.

23.—Pain of the head very violent and inceſſant; pulſe only 60, but full and regular. She frequently lay a long time without breathing, and then ſighed deeply. The pupils were ſtill dilated, and ſhe had an evident ſtrabiſmus, the axis of the right eye being directed towards the noſe. As ſhe continued to be coſtive, the glyſter was ordered to be repeated.

24.—Pulſe only 52, very regular, not quite ſo much ſtupor, but the pain of the head very violent. Strabiſmus and dilated pupils as before. Had voided a conſiderable quantity of high coloured urine during the night; ſhe had alſo begun to ſpit a good deal, but did not complain of ſoreneſs of the mouth.

25.—Pulſe 60, regular and full; leſs ſtupor; had ſlept none in the night, but ſighed frequently; complained much of the pain of her head, which ſhe ſaid was more violent than ever, and extended towards the *occiput*. As the bliſter between the ſhoulders was healed, I directed a ſmall one to be applied behind each ear. She had ejected upwards of a pint and a half of ſaliva, and voided a large quantity of urine. In order to check the ſalivation, which I was apprehenſive might go to a greater length than I wiſhed, I directed eight grains of *kali ſulphuratum* to be taken

taken every two hours in a little water. This, as I have elsewhere obſerved,* is always found very efficacious in checking or putting a ſtop to ſalivation. In 24, or at moſt 48 hours after the firſt exhibition of this remedy, the ſalivation becomes much abated. It has been reaſonably ſuppoſed that mercury derives moſt of its activity from being in the ſtate of an oxid; for crude mercury poſſeſſes little or no power, and in the form of mercurial ointment, the mercury is evidently oxygenated by continued friction. On the decompoſition of the water in which the medicine is given, by the kali ſulphuratum, ſulphurated hydrogen gas is produced, which will be conveyed into the blood, where the hydrogen unites with the oxygen of the mercurial oxid, and forms water, while the ſulphur will convert the metal into an ethiops or ſulphuret, which is very inert.

With a view of affording ſome relief to the excruciating pain of the head, I directed a pill of opium to be taken at bed time.

26.—Pulſe 80, very full and regular. Had ſlept none, but been delirious the greateſt part of the night; pain intolerable.—Strabiſmus ſtill remains, but the pupils contract a little on the approach of light. Salivation diminiſhed. The quantity of urine not ſo great, but ſtill conſiderable. The pills and mercurial ointment had been omitted ſince yeſterday. To alleviate the pain, the temples and forehead were rubbed with ether; but this proving ineffectual, three leeches were applied to each temple, and one of the following powders was directed to be taken every three hours in a little tea.

℞. Nitri purif. ʒij.—Sacchar. alb. ʒiij.—Pulv. coccinell. gr. viij. M. et divid. in part. xij.

27.—The pain of the head was relieved for ſome hours after the application of the leeches, but had returned, though not quite ſo violent as before. Pulſe 80, not quite ſo full. No perceptible ſtrabiſmus, but the pupils continue dilated. The powders were ordered to be continued.

28.—Had ſlept a little, but ſtill complained of great pain in the head. Pulſe 84, rather full. Salivation gone, urine natural. As ſhe nauſeated the nitrous powders, an effervеſcing ſaline draught was directed to be taken every four hours.

30.—Pain of the head ſtill bad, though not quite ſo acute. Pulſe 80, not ſo ſtrong. Has had a ſtool every day for the laſt three days. The medicines continued.

December 1.—Pain of the head very bad, but ſlept a little laſt

* Letter to Dr. Beddoes, publiſhed in his Conſiderations on the medicinal uſe and production of factitious Airs, p. 115. 2d edition.

laſt night. Pupils conſiderably dilated. Pulſe 96, of natural ſtrength, and regular.

The pills of calomel and digitalis were ordered to be repeated.

2.—Had two ſtools yeſterday. Slept a little during the night. Pain of the head and pulſe as yeſterday. The effervefcing draught being very grateful, was ordered to be continued as well as the pills.

4.—Pain leſs acute. Pulſe 96, and regular. Pupils not ſo much dilated, and contract more on the approach of light. Sat up three hours yeſterday. The bliſters diſcharge well. The pills were directed to be continued, and the friction with mercurial ointment to be reſumed.

6.—Pain much leſs acute; has ſat up ſeveral hours. Pulſe natural, pupils not dilated; ſlept well. Remedies to be continued.

8.—Pain gone. Complains of nothing but weakneſs. The pills and friction to be omitted.

12.—No complaint but that of being rather coſtive.

℞. Maſs. pil. Rufi ʒjſs.—Calomel. ℈j.—Syr. ſimp. q. s. ut ft. Pil. xx.—Sumat j. vel ij. pro re nata.

31.—No return of the complaints.

During the worſt period of her illneſs it may be obſerved, that ſhe never aſked for any thing; but when food of any kind was offered her, ſhe took it very eagerly. She was not afflicted with vomiting, except during a few of the firſt days.

REMARKS.

From a view of the ſymptoms in the preceding caſe, there can be little heſitation in referring them to the diſeaſe which Dr. Whytt calls *Hydrocephalus internus*; but which Dr. Cullen, perhaps with more propriety, calls *Apoplexia hydrocephalica*, though I think this term not altogether unexceptionable, for reaſons which I ſhall afterwards mention.

All the ſymptoms enumerated by this accurate Noſologiſt, in his definition, were preſent in this caſe. "Apoplexia *(hydrocephalica)*,) paulatim adoriens; infantes et impuberes, primum laſſitudine, febricula et dolore capitis, dein pulſu tardiore, pupillæ dilatatione, et ſomnolentia afficiens."*

The ancient phyſicians ſeem to have had but little knowledge of this diſeaſe, and it is even only within theſe few years that we have had any accurate accounts of it. Whether this diſeaſe is now more common than formerly, as appears to be the caſe with ſome others, I cannot determine, but am of opinion that it was generally miſtaken; and though in early medical writers, we

* Synopſis Noſologiæ Methodicæ, vol. II. p. 187.

we meet with deſcriptions of ſymptoms ſimilar to thoſe enumerated in the definition, they were thought in general to proceed from worms, and treated accordingly.

Dr. Whytt was the firſt who publiſhed any accurate deſcription of this diſorder, and of the appearances on diſſection. Afterwards, in the London Medical Obſervations and Inquiries, vol. iv. we meet with an excellent Eſſay on this diſeaſe, by the late Dr. Fothergill.

Moſt authors who have deſcribed it, agree in their account of its fatality. Sennertus ſays, "Si poſt dolorem vehementem, magna ſequatur pupillæ dilatatio, exigua ſanationis ſpes eſt."* Dr. Fothergill alſo ſays, "At the ſame time I muſt own to you, it is not in my power to ſuggeſt any probable means of curing the diſeaſe of which I treat; it has baffled all my attempts, both when confided in alone, and in conſultation with the ableſt of the Faculty. All I pretend to do is, to exhibit ſuch an idea of this diſeaſe as may ſerve to make it known when it occurs in practice, and to form ſuch a prognoſtic of its progreſs and event, as may juſtify practitioners to themſelves and to the families in which ſuch fatal occurrences may preſent themſelves."† Dr. Whytt freely owns that he has never been ſo fortunate as to cure any one patient, whoſe ſymptoms denoted its exiſtence with any degree of certainty.

Within theſe few years I have ſeen ſeveral inſtances of this malady, and though I have only been completely ſucceſsful in the preſent and two other caſes, yet there are now upon record ſeveral ſucceſsful caſes; and I am in hopes that when we become better acquainted with the nature of the diſeaſe, the ſucceſsful caſes will counterbalance thoſe of a contrary deſcription. Since the time of Dr. Fothergill, this diſeaſe has attracted the attention of ſeveral eminent phyſicians, particularly of Drs. Percival, Haygarth, Dobſon, &c. who have all favoured the public with valuable obſervations on it. The beſt account of this diſeaſe is however given us by Dr. C. W. Quin of Dublin, in "A Treatiſe on the Dropſy of the Brain." He has given a number of valuable and highly important ſuggeſtions with reſpect to the treatment as well as the theory of this diſeaſe, and ſo accurate a deſcription of it, that practitioners will now be ſcarcely at a loſs in diſtinguiſhing it. A general outline of his opinions on this ſubject was given in his Inaugural Diſſertation, publiſhed at Edinburgh in the year 1779. Since that period, he ſays, that neither facts nor arguments have occurred of ſufficient force

* Lib. I. Part 3, Sect. 2. Chap. 26. Pract.
† Med. Obſ. and Inquiries, Vol. iv.

force to weaken the conviction of their truth; on the contrary, his theory can now appear supported by an added strength of evidence, which minute attention to the subject has enabled him to draw from books, and observation in a variety of cases.

Within these last six years I have seen several instances of this disease and though only in the cases I have mentioned; have I been able to effect a cure, yet in two others the symptoms were so much alleviated by what is generally called the antiphlogistic plan, conjoined with mercurials and digitalis, that I had great hopes of their recovery. But the misfortune is, that a physician is generally consulted after the inflammatory stage is past, in which alone any good can be done.

Since Dr. Whytt favoured the world with his excellent history of this disease, I think it has been nearly a general opinion that children are almost the only subjects liable to its attacks, and chiefly those between the fourth and tenth years of their age; and I am of opinion that the symptoms which indicate the presence of this disease, seldom show themselves in an advanced period of life, but are much oftener met with in infants; yet from the observations of Dr. Quin and my own experience, this disease does not by any means appear to be confined within the limits just mentioned. The subject of this case was sixteen. I had one patient who was thirty years of age, and another thirteen. Dr. Quin relates some cases of patients forty years of age, though such instances are certainly not very common in the usual course of practice.

The proximate cause of this disease has hitherto been involved in considerable obscurity, and consequently the practice must have been guided by vague and uncertain principles. A due consideration of the various circumstances which present themselves in this disease, together with the appearances on dissection, would induce us to suspect, on very probable grounds, that the disease depends upon, and is generally, if not always, accompanied by a plethoric state of the vessels of the brain, occasioning a considerable degree of inflammation, and generally, (though not always,) producing an extravasation of watery fluid before death. As proofs of the existence of this inflamed state of the vessels of the brain, I beg leave to offer the following circumstances. In all cases where blood has been drawn, it exhibits the same appearance as in cases which are undoubtedly inflammatory. In some of the cases which fell under my observation, the blood had this appearance in as great a degree as I have ever observed in pleurisy. The acuteness of the pain and fulness of the pulse likewise countenance this supposition; but what amounts almost to a demonstration, is the aggravation of the symptoms from the exhibition

hibition of ſtimulants; and the relief which in the firſt ſtage of the diſeaſe, always attends the antiphlogiſtic or debilitating plan; both of which were ſeen in the caſe juſt related. The appearances on diſſection likewiſe confirm this. Dr. Quin mentions the appearances which preſented themſelves in the brain of a young woman who died at Edinburgh in the year 1777, and that of a boy attended by his father ſome years ago in Dublin.* In both of theſe caſes, the blood veſſels of the brain were ſo unuſually diſtended, that the whole cerebrum and cerebellum reſembled an anatomical preparation in which the utmoſt force of injection had been employed. What is very remarkable in theſe caſes, is, that though the ſymptoms preceding death were ſo unequivocal, yet in both caſes, to the aſtoniſhment of thoſe who were preſent, and who expected to find a conſiderable quantity of water in the cranium, none could be diſcovered.

In four other caſes mentioned by Dr. Quin, where the quantity of effuſed fluid found in the ventricles was conſiderable, there were ſigns ſo viſible of an increaſed flow of blood to the brain, that in all of them the veſſels were remarkably turgid, and in moſt of them a great degree of inflammation had actually taken place.

The accurate Morgagni alſo relates ſeveral inſtances in which the ſymptoms of this diſeaſe had been preſent, and where, upon diſſection, evident marks of morbid accumulation, and of an inflammatory ſtate of the veſſels of the brain, had appeared.†

All theſe circumſtances ſtrongly tend to confirm the ſuppoſition that the ſymptoms, particularly thoſe which appear in the firſt ſtage of *Apoplexia Hydrocephalica*, depend upon a plethoric and inflammatory ſtate of the veſſels of the brain. And though in many caſes, upon diſſection, a quantity of fluid has been found in the ventricles of the brain, yet may not this be looked upon as a conſequence of the diſeaſe, rather than as the cauſe of it? And is it not probable that this watery effuſion does not take place till the veſſels have been oppreſſed for ſome time by the exceſſive congeſtion of blood, and in conſequence of this, and the violent excitement, have loſt their tone; whence the aqueous fluids are diſcharged in conſiderable quantities into the adjacent cavities, while the abſorbents, probably weakened by the ſame cauſe, are unable to abſorb

* Quin on Dropſy of the Brain, p. 50.

† Vide Morgagni de Cauſis et Sedibus Morborum, Epiſt. i. Art. 2, Epiſt. vi. Art. 6, and Epiſt. i. Art. 14.

abſorb the fluid as faſt as it is effuſed? It appears to me even doubtful whether the ſymptoms which generally ſhow themſelves in this diſeaſe, depend upon the effuſed fluid. Many of them undoubtedly do not; and in the two caſes related by Dr. Quin, "all the ſymptoms that generally attend this diſeaſe were preſent, and yet upon diſſection, to the aſtoniſhment of the ſpectators, who were prepared to find a great redundancy of water within the cranium, none could be diſcovered in any part of the brain." Surely, the name *hydrocephalus* is not applicable to ſuch caſes.

In every caſe therefore, it would be proper in the firſt ſtage, if no ſymptoms particularly forbid, to uſe the debilitating plan to a conſiderable extent. I have made uſe of it in three caſes which have been cured, and have ſeen others much relieved by it; at the ſame time however it muſt be noticed, that the other remedies ſoon to be mentioned, were adminiſtered, for the diſeaſe generally runs on ſo rapidly to its fatal period, that I think it would not be juſtifiable to neglect any of the means likely to arreſt its progreſs.

In ſtrong ſubjects, eſpecially thoſe who have attained the age of puberty, general bleeding may be uſed; but I ſhould rather prefer local bleeding, becauſe eventually a great degree of debility comes on; and it ſeems probable that the accumulation in the veſſels of the head may be moſt effectually relieved by evacuations made as near the part as poſſible. I am convinced, that there often exiſts, not only in this diſeaſe but in ſome others of the inflammatory kind, a local inflammation without much general ſthenic diatheſis; and when the veſſels have taken on an inflammatory action in any part, general bleeding may be employed to ſuch an extent as to weaken the ſyſtem very much, without conſiderably abating the increaſed action, while local bleedings, eſpecially after one general evacuation, often produce a ſpeedy ſolution of the inflammation. This is the caſe particularly with the acute rheumatiſm.

In ſome caſes it might be adviſable to open the jugular vein, or even the temporal artery, but I have never yet ſeen either of theſe operations performed in this diſeaſe. As in almoſt all caſes, coſtiveneſs takes place, and aggravates the ſymptoms, it will be right to keep the bowels open by means of purgatives exhibited by the mouth, or ſtimulant glyſters. In all caſes I have had recourſe to bliſters, and I think with advantage. Where it has not been objected to, I have applied a bliſter to the whole head; in other caſes I have directed them to be applied behind the ears, or between the ſhoulders.

The greateſt chance of cure is during the inflammatory ſtage, and by means of the antiphlogiſtic regimen carried to its

its fulleſt extent. Whether in this ſtage much is to be expected from mercurials, I cannot ſay. They are highly ſpoken of by Drs. Percival and Dobſon in the Medical Commentaries; and on account of this reſpectable authority, I have always preſcribed them, as ſoon as the nature of the diſeaſe could be aſcertained. That mercurials have been employed with ſucceſs appears likewiſe from the teſtimony of Dr. Moſeley.* Alſo from that of Drs. Hunter and Haygarth.† Is it not probable that this remedy may produce its good effects by exciting the action of the abſorbents, and thus enabling them to take up the extravaſated fluid? and would they have produced any good effects in the two caſes related by Dr. Quin, where there was no watery effuſion? The digitalis being one of the moſt powerful diuretic remedies with which we are acquainted, is a very proper remedy in this diſeaſe; and I think I have found that its diuretic effects are much more certain and powerful, when conjoined with mercury. The digitalis likewiſe claims attention on other grounds. It ſeems to poſſeſs the power of diminiſhing the action of the arterial ſyſtem in a very remarkable degree, and is therefore indicated in the inflammatory ſtage.

From the attention which phyſicians have of late years paid to this diſeaſe, we ſhall I hope be ſhortly in poſſeſſion of ſufficient facts to eſtabliſh a rational theory of it, which will lead to a ſucceſsful plan of cure. Should theſe remarks contribute ever ſo little to the attainment of ſo deſirable an end, I ſhall be highly gratified.

THOMAS GARNETT.

To the Editors of the Medical and Phyſical Journal.

Gentlemen,

By enquiry and diſcovery, the condition of man is improved; and by the continued application of talent and induſtry to the art of healing, it is to be expected that much will be atchieved. The old fabrics of phyſic will undergo a cloſe examination; the rubbiſh will be removed, and the valuable materials only will be ſuffered to remain: the myſterious pe-

* London Medical Journal, Vol. vi. p. 113.
† Medical Obſervations and Inquiries, Vol. vi.

dantry of the antient ſchools, and the blind devotion to antient ſyſtems, will be doomed to contempt; and medical opinions will command veneration, not becauſe of their antiquity, but becauſe they are proved to be true. The profeſſors of medicine are now acquiring additional knowledge of the mechaniſm of the human ſyſtem; and by the aid of obſervation and experiment, they are purſuing inquiries which will lead, perhaps, more directly to the cure of diſeaſe. The true end of ſcience is the production of new powers, and the means of applying them to uſeful purpoſes; and the unfolding of powers, which will infallibly leſſen the accumulated maſs of human evil, is an object of high importance. The votaries of medicine then, ſhould never be idle; for induſtry or accident may eventually teach them to ſubdue maladies, which now elude every exertion of their art. There was a period when *ague* and *ſyphylis* were incurable: the antidote to both is now well known. It is certainly compatible with probability, and I believe it to be true, that Nature holds a remedy for every evil by which we are aſſailed; and, perhaps, the time is not far diſtant, when there will be a diſcovery of powers, which ſhall very generally obviate the fatality of *fever* and *pulmonary conſumption.* In the primary ſtages of *phthiſis*, the *digitalis* certainly approximates to a ſpecific; and, perhaps, when properly adminiſtered, or when aided by proper auxiliaries, it is poſſible that the plant may operate as a curative agent, even in the advanced ſtages of that diſeaſe. *For fever there is no remedy.* In this iſland, at leaſt, there never has been any power employed, which could, with any certainty, arreſt the progreſs of *fever* for a ſingle day. If this obſervation be conſiſtent with univerſal experience, let it apologize for the freedom of the ſubſequent diſcuſſion: If the progreſs of intellect be daily productive of beneficial inventions, ſurely, a ſcience which embraces ſo large a portion of the fate of man, cannot be ſtationary. If the combined ravages of *fever* and *pulmonary conſumption* are deſtructive of more than half of thoſe who periſh by diſeaſe, it is very certainly an object of much moment to obviate their fatal tendency; and the accompliſhment of this end, ought alone to be the limit of inquiry. The extended uſe of that invaluable plant, which is found to govern the heart, and to exhibit powers ſingularly curative in certain ſtates of *pulmonary conſumption*, is a recent diſcovery; and is it impoſſible that there alſo exiſts a power completely ſubverſive of the phænomena of fever? This is an inquiry of extreme importance, and nothing can be loſt by purſuing it. It is known that the preſence of *fever* is conſtituted or indicated by an increaſed action of the ſanguiferous ſyſtem; but it is not known what is the

the remote and exciting cause of this morbid action; nor is it known why it should be so permanent. It is, I believe, consistent with the knowledge of every physician, that brisk *evacuations* recurred to on the first accession of *fever*, will sometimes be subversive of the *febrile action*; but I am persuaded it is also true, when this period is gone by, that the practitioner cannot maintain any other character than that of an observer. Although there are various doctrines of the nature and cure of *fever*, yet, respecting its leading features, there is a very general coincidence of opinion; and the more obvious phænomena of *fever* have been so strikingly uniform, as to produce descriptions very exactly similar. I have already observed, that the operation of brisk *evacuants* has been sometimes found to be subversive of *febrile action*; but if those powers be ineffectually employed, the morbid catenation will continue for an unlimited period, in defiance of every effort of medical interference. During the continuance of *fever*, the physician may, in the character of an observer, direct what he conceives to be a salutary regimen; he may also, perhaps, be able to promote some of the natural discharges, or to correct them, if they be morbidly profuse; but this, I apprehend, is the extent of his power. For an indefinite number of days or weeks, a chain of morbid action will continue to be exhibited, till a *crisis* or *solution* of the *fever* is effected, by means of which we know nothing. This mode of the termination of *fever*, however, is by no means a constant appearance; for it very frequently happens, that the morbid action gradually rises to a certain degree, without any striking exacerbation of symptoms, and then begins to decline imperceptibly, till it entirely disappear. If those phænomena continue to be exhibited, unconnected with, and independent of, medical exertion, it is surely incorrect to speak of the *cure of fever*.

About twelve years ago I published Observations upon an Epidemic Fever, in which I employed, what may be denominated, the *Brunonian instruments*; and I was much gratified, by having the recovery of cases, apparently desperate, ascribed to my professional skill. Youth and inexperience persuaded me, that I was in possession of artillery very certainly destructive of *fever*. A few failures, however, soon afterwards occurred to confirm my unacquaintance with the nature of *fever*, and the inefficacy of my curative agents. I turned over the pages of authors, but they did not afford me satisfactory information on the subject: I again had recourse to my own observations; and by comparing what I read with what I saw, I became well persuaded, that *febrile action* was a phænomenon, neither understood, nor curable by the means then

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employed. I am here to be underſtood as ſpeaking of *idiopathic fever*, or *fever ariſing from contagion*. In conſulting the various diſſertations on *fever*, I found one author extolling one remedy, and another man giving praiſe to another; I employed all thoſe boaſted remedies ſingly; I varied them, and I combined them; *evacuants*, *refrigerants*, *diaphoretics*, *tonics*, *antiſceptics*, *ſtimulants*, and *anodynes*, had all an impartial trial; and under every mode of treatment, ſome of my patients periſhed, and ſome recovered; nor did the iſſue appear to be effected by any exertion of mine. The following general concluſions, however, then forced themſelves upon my notice; 1ſt. that the application of powers repreſſive of arterial action, was manifeſtly beneficial; and, 2dly. that the application of powers of a contrary tendency, was as manifeſtly injurious. I do admit, that ſome of the moſt diſtreſſing ſymptoms of *fever* may be alleviated, and ſometimes completely obviated by the inſtruments of medicine; yet, I very ſincerely believe, that could there be a juſt eſtimate of the good and the ill occaſioned by medical interference in *fevers*, I have no heſitation in aſſerting, that the balance will be found to be againſt our art. My experience has been too ample for me to miſtake; and I am well perſuaded, that a greater number of patients labouring under *continued fever*, will recover by being treated with *toaſt and water*, than by an exhibition of the remedies now in uſe.

We are not yet ſufficiently acquainted with the laws which direct the ſyſtem in a ſtate of health, and we know ſtill leſs of the circumſtances which influence it in a ſtate of diſeaſe; but I am convinced, that if the cure of *fever* be practicable, it is to be effected either *by exciting an action ſubverſive of the one preſent*, or *by directly arreſting the catenation of diſeaſe*. I believe it is true, that there are degrees of morbid action, and that a greater action is ſubverſive of an action leſs powerful. This doctrine is ſupported by authorities the moſt reſpectable: It has been obſerved, that one morbid action only can operate in the ſyſtem at the ſame time; and if two contagions be received, that the leſſer remains in a latent ſtate, till the greater either ceaſes to act, or is nearly exhauſted. It has further been obſerved, that the morbid action of the *plague* baniſhes from the city or place where it prevails, all inferior epidemic action. It is alſo true, that at the approach of the *yellow fever* in extreme malignity, all other epidemics inſtantly diſappear: it is conſiſtent too with the obſervations of every one, that the phænomena of *mania* and *pregnancy* will very conſtantly impede the progreſs of *pulmonary conſumption*. An additional ſpecification is unneceſſary; for, if what I have ſtated be true,

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it is impoſſible not to pauſe! Theſe facts may afford uſeful information. The evidence reſpecting the curative operation of *calomel* in the *yellow fever*, which prevailed in *Philadelphia*, is too ample to admit of doubt; and I am ſtrongly inclined to believe, that the circumſtance of its exciting a new action, will account for its beneficial effects. It is well known, that *mercury* is deſtructive of the *ſyphilitic virus*, and that as ſoon as the action of the former is apparent, the ſymptoms of the latter begin to diſappear. I know too, that by means of *mercury*, I can hold in check the action of the *variolous contagion*, and by the ſame power render the diſeaſe, even in its worſt form, very certainly mild: it is impoſſible to know thoſe facts, and not to draw inferences. I make little account of the ſuppoſed action of *calomel*, as an *evacuant*; for it is certainly true, that medicines poſſeſſing powers very ſtrongly purgative, have been fruitleſſly employed in checking the progreſs of *fever*; nor do I ſubſcribe to any ſuppoſed chemical action of *mercury* on the ſyſtem of fluids; for, if that ſuppoſition be juſt, there are means infinitely more eligible for the accompliſhment of the end propoſed. There are, perhaps, other powers which may be ſucceſsfully employed to fulfil my intention of the cure of *fever*; but I think it probable, that *mercury* will continue to hold the firſt place. For the purpoſe of directly arreſting the catenation of diſeaſe, I recommend the *digitalis:* the power of this plant over the action of the heart, is too well known to need any comment. The *ſymptomatic fever* accompanying *pulmonary conſumption*, is ſo perfectly under the management of the *foxglove*, that the phyſician may keep the patient's pulſe at any given number, and thereby be enabled to adminiſter tonic or other remedies radically curative of the complaint.

I am now employed in making obſervations upon the ſubject of curing *fever* by the means I have recommended, and I hope ſoon to be enabled to lay before you the reſult of my experience. I have been too often diſappointed to feel extremely ſanguine; but if my future trials correſpond with the advantages which I think I have already gained, I am perſuaded that the practice will add ſomething valuable to the treaſury of medicine. I am,

GENTLEMEN,

Your's, moſt reſpectfully,

Bradford, Nov. 24, 1800. G. MOSSMAN.

To

To the Editors of the Medical and Physical Journal.

GENTLEMEN,

I Obſerve that diſcuſſions on the Cow-pox ſtill continue to occupy ſeveral pages in each Number of your Journal. Perhaps no ſubject ever met with ſo ample an examination within ſo ſhort a time; and indeed its importance to the welfare of mankind is commenſurate to the pains beſtowed upon it. The real merits of the diſcovery, however, ſeem to lie within a narrow compaſs, and to reſt chiefly on two points: Firſt, whether it be a ſecurity againſt the Small-pox? and, ſecondly, whether it be a milder diſeaſe? If the teſtimony of medical men in its favour, more general than ever was before publiſhed on any one profeſſional ſubject, can be ſuppoſed to determine the former, it muſt be admitted as proved beyond all controverſy; and, with regard to the latter, the reſult of unambiguous experience is alike convincing. Theſe two points then being as ſatisfactorily proved as any others in medicine, it is unfair to charge the diſcovery itſelf with miſtakes which have occurred in conducting the inoculation.

The ſources of error in inoculating for the Cow-pox, appear to have ariſen from miſtaking the proper time for taking the fluid, and by not noting the characteriſtic progreſs of the diſeaſe on the part inoculated. In my own practice I have obſerved, that the veſicle contained the largeſt quantity of fluid on the tenth day, including that of inoculation, and, on the eleventh, that it had nearly diſappeared, and what little remained had become inert; at leaſt, when taken in that ſtage, it has failed with me in exciting the diſeaſe, in the ſeveral inſtances in which I have tried it. There is ſome variety however in the courſe of the diſeaſe, and the ninth day may, in ſome caſes, be more proper for taking it, as, in others, I have found the eleventh and twelfth to be. About a month ſince I inoculated a boy for the Cow-pox, and till the tenth day I was not certain that he had taken the infection; the changes on the arm were thence very rapid, and no fluid could be diſcerned in the veſicle on the thirteenth. His two brothers, both younger than himſelf, were inoculated from the ſame thread, at the ſame time, and went through the diſeaſe in the uſual way.

Much apprehenſion has been entertained of an extenſive inflammation ſpreading from the inoculated part, and of a conſequent ulceration. In my own practice, the inflammation has ſometimes been conſiderable, but it has not furniſhed me with a ſingle

a ſingle inſtance of a troubleſome degree of ulceration. In ſeveral, where the inflammatory appearances have been threatening, I have thought that they have been aggravated by the tightneſs of the dreſs, and accordingly on ordering a looſe veſt, they have diſappeared in due time. But in the worſt caſes I have yet ſeen, no other application has been neceſſary than the vegeto-mineral water.

An eruption of puſtules was, for ſome time, a pretty conſtant attendant on this inoculation; latterly, however, ſeldom any have appeared on diſtant parts of the body, and not more in number than three or four; the inflammation on the arm, together with a puſtular eruption on that part, and a ſlight febrile attack, have conſtituted all the ſigns of diſeaſe. The true character of the Cow-pox had undoubtedly been changed by an intermixture of the variolous and vaccine virus.

Frequent diſappointment has ariſen from the fluid employed for inoculation having loſt its activity; this may be obviated in future, by inoculating immediately from the ripe veſicle; or, where that cannot be complied with, the chance of miſcarriage may be leſſened by ſaturating a thread, and keeping it in a well-corked vial till the time of uſing; for, when taken upon a lancet it ſoon becomes inert. There is a ſtriking difference in this reſpect, between variolous matter and the vaccine; which I have been accuſtomed to explain by ſuppoſing that, in the Small-pox, the active particles are ſo enveloped in pus, as to prevent their ſudden exhalation or decompoſition; whereas, in the Cow-pox, no ſuppuration takes place, and the fluid being but little more viſcid than water, is ſoon deprived of its peculiar properties by the ſpecific portion flying off, or by undergoing ſome chemical alteration.

Though no inſtance of a violent or unmanageable degree of inflammation or ulceration has occurred to me, the experience of others proves, that both are ſometimes very troubleſome and alarming. As I cannot write upon this point from my own obſervation, I ſhould have ſtopped here; but, reaſoning from analogy, I am led to ſuſpect, that in thoſe caſes where either one or other of the above ſymptoms, particularly the former, has taken place in a violent degree, the future ſecurity of the patient againſt the variolous contagion muſt be rendered ſomewhat doubtful. The inflammation on the part, and ſymptoms of general irritation deſcribed by writers, reſemble thoſe produced by the abſorption of putrid matter from a wound, or what are cauſed by a ſimple inciſion in a bad habit of body, and are eſſentially different from the regular courſe of the diſeaſe. Theſe bear a cloſe analogy to a chancre happening in a bad conſtitution, and followed by inflammation terminating in

in gangrene, or in an ulcer difficult of cure. How violent ſoever theſe ſymptoms may be, the ſyſtem not uncommonly eſcapes contamination by the venereal poiſon; and therefore I hold it ſafe, when the inflammatory appearances on the arm greatly exceed the uſual limits, that the Vaccine Inoculation ſhould be repeated at a convenient diſtance of time, to guard againſt all chance of diſappointment. It is only after the changes upon the inoculated arm have been regular and well defined, that a patient can be ſaid to have ſecurely undergone the diſeaſe.

On the origin of the Cow-pox, conjecture has been buſily employed; but, whether it originate in the horſe or in the cow, or by the united influence of both, does not appear to be of much practical importance. The objections derived from the loathſomeneſs of the origin of it in the horſe, would, by parity of reaſoning, reject the uſe of pork and of duck for food, becauſe ſwine and ducks are foul feeders. It is not the ſource whence it may be derived, ſo much as the uſeful purpoſes to which it may be applied in its mature ſtate that claim our attention. We ſhould be little ſolicitous whether mercury were derived from the animal or vegetable inſtead of the mineral kingdom, whilſt we know that it is an antidote to the venereal poiſon. However, as an object of curioſity, and as opening a new field for phyſiological inquiry, the ſuggeſtion may be ſubmitted again to the teſt of experiment, the only mode of aſcertaining its truth or fallacy. In thoſe experiments which I publiſhed on this ſubject two years ago, every attention of which I am capable was paid to accuracy, and the reſult was decidedly againſt that opinion. Lately, the matter of greaſe has been ſaid to have been employed with ſucceſs; but ſurely where the genuine Cow-pox fluid, of any age, had been ſuffered to come in contact with the ulcerated ſurface, it muſt throw an ambiguity over the whole. An experiment ſo looſely conducted, may deſerve repetition, from the reported contradicton of former facts, and the ſeeming ſingularity of the reſult; but cannot be deemed concluſive againſt the experiments of others, as well as myſelf to the contrary. Nevertheleſs, I am not at all tenacious of my opinion, and ſhall readily relinquiſh it, on ſeeing proper grounds for a conviction of its error.

Beſides the two cardinal points of ſecurity and mildneſs, the Cow-pox poſſeſſes other properties of a very uſeful though ſubordinate nature; it is not contagious; it neither ſtirs up ſcrofula, nor any other diſpoſition to diſeaſe that may lie dormant in the ſyſtem; nor with itſelf is any other introduced; and it may be communicated at all ages, and under all circumſtances

in which a rational practitioner would think of communicating it. By the general introduction of this practice, the comfortable hope of exterminating the Small-pox may be reasonably entertained, and thus millions of lives yet unborn be preserved to their friends and society.

We learn by the accounts from the Mediterranean, that the Cow-pox preserves its characteristic mildness even in a more southern latitude; where, when it appears, the Small-pox is dreadfully destructive. This is particularly the case upon the coast of Africa, and on board our slave-ships; to obviate which in future, I would recommend the introduction of the Cow-pox, and that every slave should be inoculated for it, as soon as taken on board. By this means their lives would be preserved, and their value enhanced by the security, when arrived at the place of their destination. The most beneficial consequences might be expected also from introducing it among the slaves in the West Indies, as the Small-pox is likewise very destructive to them in that climate.

I have put together these few observations, in order to add my testimony in detail, in favour of the new practice, to the mass of evidence already before the public; and to pay my tribute of respect thus publicly to Dr. Jenner for the original suggestion, and to Dr. Pearson and others for the zeal and industry with which they have promoted the practice of this valuable discovery. Should they be deemed of sufficient importance to merit a place in your Journal, they are entirely at your disposal. I am, &c.

W. SIMMONS.

Manchester,
December 9, 1800.

To the Editors of the Medical and Physical Journal.

GENTLEMEN,

AT the conclusion of a paper honoured with insertion in your valuable Miscellany (No. XIX.) about three months since, I signified my intention of treating more largely on the benefit which is derived from the exhibition of the Artificial Musk in Hooping Cough, as well as in cases of extreme debility. Leisure now permitting me to make a few observations on the subject I, without further apology, submit them to your notice.

It would be unneceſſary to make a full and tedious explanation of thoſe caſes of hooping cough relieved by the muſk; it will be ſufficient to ſay, that I have adminiſtered it in ſeveral, and with conſtant and uniform good ſucceſs. The urgent ſymptoms are ſpeedily mitigated by this powerful remedy, and the diſeaſe is quickly ſubdued, as it were, by a ſpecific action, unindebted to the aid of any other medicine, I never having preceded its exhibition, even with an emetic: Doubtleſſly, others who have adopted its uſe, muſt with me have had the ſatisfaction of noticing its good effects. I think it neceſſary to obſerve, that I have conſtantly introduced the medicine in form of a tincture, prepared as I have already directed in my laſt paper: This has, I think, the advantage of a more ſpeedy efficacy, as being more diffuſive, and is leſs likely to offend the ſtomach, than when taken in ſubſtance or in an emulſion, for I have obſerved the latter frequently to create nauſea. Conſidering twenty drops of the tincture as a ſtandard doſe for an adult, I proportionate the quantity to be taken according to the age, temperament, &c. of my patient. This is, I think, as much as is neceſſary for me to ſay on this part of the ſubject: I ſhall, therefore, proceed to tranſcribe a caſe, wherein the tonic and reſtorative powers of the muſk are ſo prominent, that I think it deſerves to be recorded.

Eleanor Sadler, aged about 23 years, a pauper of this pariſh, of a light, ſanguine temperament, and a habit prediſpoſed to ſcrophula, had been from her earlieſt infancy the unhappy victim of diſeaſe. When very young, the ſmall-pox deprived her of the ſight of one eye, and greatly endangered the other. She thus continued almoſt in a ſtate of blindneſs, until the age when catamenia uſually begin to appear, at which time the miſery of her condition was much aggravated, as every method taken within her power, for the promotion of the ſecretion, proved ineffectual. She was ſhortly afterwards affected with acute pain throughout the whole epigaſtric region, attended with a dyſpnœa ſo violent, as almoſt to preclude the poſſibility of the leaſt motion or exertion without endangering ſuffocation. By applying to the pariſh ſurgeon for relief, ſhe obtained a temporary one by venæſection; but, although this was frequently repeated, reſpiration ſtill continued ſo laborious, and the pain ſo troubleſome, that ſhe could not in any degree contribute towards providing a ſcanty pittance for her ſubſiſtence. She continued in this deplorable condition for many years, notwithſtanding Mr. Hooper, (the ſurgeon before alluded to) with the greateſt humanity and attention, endeavoured by every mean within his power to *mitigate* her ſufferings, having relinquiſhed the hope of entirely *removing* them. He at length adviſed her

to

to go to the Briſtol Infirmary, and procured a recommendation for her. From thence ſhe was ſoon diſmiſſed as incurable. During her ſtay there, ſetons were repeatedly made in her temple, to remove a pain ſhe felt in her remaining eye, the anguiſh of which was almoſt inſupportable, there conſtantly exuding from it an ichorous corroſive diſcharge, except when removed by an artificial drain. Soon after her return to this place, another perſon was nominated to the office of pariſh ſurgeon, who frankly told her, he could do nothing to relieve her, nor would he attempt it. Thus, deſerted by the only aſſiſtance her extreme poverty could claim, ſhe ventured to intreat my attention to her caſe: I complied with her requeſt, and examined her eye, but could diſcover no external inflammation, either in the tunica conjunctiva or elſewhere, nor any appearance of caligo; a thin acrid fluid conſtantly diſtilled in conſiderable quantities from under the palpebræ, but I could not diſcover its ſource. I had recourſe to (as before practiſed) the ſeton in the adjoining temple, and frequently ordered the uſe of collyria with opium, camphor, &c. but the relief theſe afforded were very tranſient. I alſo tried every method I could ſuggeſt to remove the dyſpnœa, which every day became more alarming; I introduced at different times almoſt the whole tribe of demulcents, tonics, alteratives, chalybeates, &c. but all without any permanent advantage. I became at length almoſt weary of the coſt and trouble I continually experienced on her account, without having in proſpect the ſatisfaction of eventually finding my pains requited by her recovery. When, in May laſt, I determined, as a "derniere reſort," to exhibit the artificial muſk, though I had little or no hope of its being attended with ſucceſs. I gave her ſome of the tincture, and directed her to take twenty or thirty drops bis terve de die; but how agreeably was I ſurprized to find, in the courſe of a fortnight, her dyſpnœa had left her; the ſight of her eye was perfectly reſtored, without pain or diſcharge; the ſeton healed up; and, in ſhort, ſhe was enabled to attend to her buſineſs, (a picker of wool) which ſhe has continued to do without intermiſſion to the preſent time, except, that about the latter end of the ſummer ſhe complained of a trifling cough and dyſpnœa, which was moſt probably occaſioned by the ſudden tranſition obſervable at that time, from a warm and dry temperature to a cold and humid one: this was, however, immediately relieved, by having recourſe again to the tincture. It may be worthy of remark, that on the pain and dyſpnœa firſt leaving her, an eryſipelatous inflammation, with a watery eruption, appeared round the whole ſurface of her lower extremities: On this occaſion, I directed her to ſprinkle ſome powdered chalk frequently

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quently on the parts, which ſoon ſubdued this unpleaſant occurrence. Soon afterwards the catamenia began for the firſt time to appear, and has continued ever ſince at regular periods, though the diſcharge is inconſiderable in quantity, and of a ſerous appearance, being diveſted of the florid colour it uſually aſſumes. I have thus given you a brief and imperfect account of a caſe, wherein I have enjoyed the felicity of reſcuing a fellow-creature from undeſcribable miſery, ariſing from extreme bodily diſeaſe and the equally ſevere pangs of wretched poverty. Her gratitude on this occaſion is ſuch as might be imagined, from one who is unexpectedly enabled to procure a humble though ſufficient ſubſiſtence, independent of parochial aſſiſtance, and not impelled by want to ſolicit the cold relief of caſual charity.

I ſhall now advert to the advantage derived from its external application to old morbid ill-conditioned ulcers. One caſe only the preſent time will permit me to ſtate.

A young girl, aged 13, of Box pariſh in this county, more than a year ſince, was ſent to me with a large ſcrophulous ulcer in the leg, of two years ſtanding. I employed, for a conſiderable time, every method in my power to heal it; but in vain I attempted to alter its unhealthy appearance, or promote granulation in any degree. I ſteadily purſued for ſome time Mr. Baynton's plan, with ſtrips of adheſive plaſter, and applied tight bandages, but with no effect. I frequently ſcarified the ſurface of the ulcer, hoping, by exciting inflammation, to produce a diſpoſition to granulate. I ſprinkled it alſo with pulv. rhabarb. and many other ſtimulant ſubſtances, which only induced a conſiderable degree of pain, without promoting in the leaſt the benefit intended. Recollecting at laſt that I had frequently heard of the good effects of oxygene applied to inveterate ulcers, and conſidering the muſk to contain a portion from the combination of the nitric acid with the oil of amber, I determined to make trial of it; I therefore daily waſhed the ulcer with the tincture before deſcribed, ſtill continuing the uſe of the adheſive. I was ſoon happy in obſerving effects the moſt deſirable; full and healthy granulations quickly formed; the ulcer aſſuming a florid, healthy appearance, healed gradually in the moſt complete manner, and the new-formed cutis ſeems firm and compact, nor does there appear any probability of a relapſe. I am, &c.

Bradford, Nov. 20, 1800. O. W. BARTLEY, Surgeon.

To

To the Editors of the Medical and Phyfical Journal.

GENTLEMEN,

TO a great majority of your readers, a defcription of the Hooping-cough would be totally unneceffary; I beg, however, through the medium of your ufeful Journal, to offer to their confideration, what I conceive will be found a very valuable remedy for this difeafe.

My attention has been turned towards this fubject, on account of its having been twice epidemic in this town during the laft eight years; and becaufe, during thefe times, I have heard it very frequently mentioned as one of the opprobria medicorum.—I have made trial of feveral different medicines.

Emetics (the ufual remedy) have never appeared to me to afford more than a very temporary relief.

I have given the calx of zinc in large and frequent dofes, fo as to excite continual naufea, with no beneficial effects, excepting towards the latter end of one folitary inftance, when the difeafe appeared to have become purely fpafmodic. In this cafe it performed a complete cure.

The flores cardam. pratens. in fine powder, appeared to have fome effect in the fame way; but on account of the great quantity that muft be exhibited for this purpofe, they are generally inadmiffible.

In a few inftances I have cautioufly ufed volatile alk. and opium; but as this medicine never failed to aggravate the morbid difpofition of the cheft, it was quickly laid afide.

Bliftering the fternum has often relieved dyfpnœa when prefent, but never feemed to have much effect upon the cough.

Having the misfortune to lofe the firft little patient who came under my care for this difeafe laft Auguft, under the common mode of treatment, and having witneffed the good effects of the Digitalis in feveral affections of the cheft, I was induced to make trial of it.

The faturated tincture, mixed with an equal quantity of fyrup, is pleafant to the tafte, and I have hitherto experienced little or no difficulty on this account; a circumftance of fome importance where children are concerned.

Three or four cafes remain yet undecided. Of feventeen others, whofe names are now before me, thirteen have been completely cured by this medicine alone. In four others it was totally ufelefs; two of thefe had mefenteric difeafe, another was teething, and the fourth, a boy aged three years, enjoying apparent health in other refpects, never was brought under the influence of the medicine, though given to the extent of 40 drops three times a day; it was increafed no further,

ther, his mother growing weary of the trouble of uselefs exhibition. So that in three of thefe cafes the failure can hardly be afcribed to the inefficacy of the remedy, as they died when it had been given a very fhort time.

The action of the medicine has not appeared to be fo speedy, in children of delicate and irritable habits, as in the more vigorous and robuft.

It has never been exhibited fo as to produce perceptible ficknefs or vertigo. Beginning with a fmall dofe, it has been gradually increafed fo as to lower and leffen the frequency of the pulfe, and continued in this manner until the cough entirely ceafed.

The age of the children on whom thefe trials were made, was from eight months to four years.

In plethoric habits I have prefcribed bleeding with one or two leeches, and have begun the ufe of the tincture immediately afterwards.

Avoiding expofure to cold, particularly keeping the extremities warm, has been fedulously recommended, and has, perhaps, been a great means of preventing a recurrence of the difeafe, a circumftance very common, more efpecially at this feafon of the year; when, however, from imprudent expofure, it has recurred (as happened in one inftance) a repetition of the tincture has been attended with the beft effects.

If the above hints fhould induce practitioners to give this remedy a fair trial, and if it fhould prove fo beneficial in this hitherto almoft unconquerable complaint, as thefe inftances feem to indicate, I fhall be highly gratified.

Whilft upon this fubject, it may not be amifs to defcribe what appeared to me a fingular termination of this difeafe.

In the winter of the year 1796, I attended a child, aged five months, for the fpace of feveral weeks, and, notwithftanding the ufe of every remedy which afforded a probability of fuccefs, it continued to become gradually worfe, until its life was defpaired of. I faw it one evening, and had no expectation of feeing it alive the next day. The following morning, however, I was agreeably furprifed to find it much better; a plentiful eruption of meafles* had taken place during the night, the cough and breathing were greatly relieved, and the child had fucked very heartily. The meafles difappeared at the ufual time, the cuticle difquamated, and, from this period, every morbid fymptom vanifhed. I remain, &c.

Hull, Dec. 26, 1800. GEO. FIELDING, Surgeon.

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* The meafles were exceedingly prevalent at that time all over the neighbourhood.

To the Editors of the Medical and Physical Journal.

GENTLEMEN,

THE impartiality with which your Journal is conducted, leaves me no room to doubt of your favouring me with an early insertion of the following Remarks, in reply to Mr. Grose, on the subject of "Yeast in Typhus."

Mr. Grose is much to be commended for investigating the subject by such experiments as would enable him to draw any useful and satisfactory inferences from them; but I think all he has done and said would have met the public eye with equal propriety, if he had not indirectly charged me with being a "Persecutor,"* and insinuated a suspicion of my candour and veracity, by observing, that all who have written in your Journal, on the subject of yeast in typhus, "with the exception of *one* gentleman, have *impartially confessed* the beneficial effects." A conduct like this is unworthy a man, who professes to be searching after truth, and is equally indifferent whether it be ultimately found by himself or his neighbour, provided he do but get into possession of it. The passage in my former letter, which seems to have given offence to Mr. Grose, is that wherein I mention the purgative effects produced by the yeast in the cases which I related.† Authors seldom have justice done them, when select passages alone are considered by their Readers; it is only by a comprehensive view of the *whole* that the true sense of any publication is to be obtained. I remember when a literary character *was tried a few years ago by one of our Universities, for publishing an offensive book*, he complained that the Prosecutor, by selecting passages, destroyed the sense of it, and made him speak things he never intended. "In this way," said he, "I can prove that the Scriptures command you to commit suicide; for in one part of the Bible it is said, 'Ahitophel went and hanged himself;' and in another, 'Go thou, and do likewise." Thus has Mr. Grose certainly misconceived the purport of my former letter, by fixing his eye too long upon a particular passage; whether owing to the obscurity of my style, or his own inattention, is not for me to determine. But I flatter myself, if Mr. Grose will oblige me so far as to give my letter another perusal, he will plainly discover that my chief design is to obviate the *danger of empiricism* in general, and not to *persecute*, with unrelenting malignity, the judicious exhibition

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* See Mr. Grose's Letter in No. XXII.

† See Postscript to Mr. Grose's Letter in No. XXI.

exhibition of yeaſt; for I even allowed, "that the fix'd air in balm may occaſionally have ſome good effect in putrid diſeaſes." My objections were, and ſtill are, againſt the *indiſcriminate* uſe of yeaſt *alone* in putrid fevers, and holding it out amongſt the vulgar, in the moſt unqualified manner, as an infallible ſpecific; as was done in the Armenian and Monthly Magazine, referred to in my letter. Is it poſſible that Mr. Groſe himſelf can believe that a perſon "in the laſt ſtage of putrid fever," was ſeen the next morning after taking ſome yeaſt, "walking in his garden," apparently well? I do not doubt the veracity of the author of this aſſertion, but I call his judgment in queſtion. If ſuch were the effects of the yeaſt, I cannot believe that the patient was in the laſt ſtage of putrid fever, eſpecially when it is conſidered, that he was "an old man upwards of 70."*

Now, as the Armenian or Methodiſts Magazine is very extenſively circulated amongſt the lower orders in all parts of the kingdom, ſurely there is a propriety in condemning the *unqualified* encomiums of any medicine as a remedy for putrid fevers. It is obſervable, however, that even in *these strong cases related by the* Rev. Mr. Cartwright, *bark* was conjoined with the uſe of yeaſt, and therefore the concluſion that the latter was a ſpecific, is not juſt; the cure may as well be attributed to the bark. So likewiſe in the ſolitary caſe with which Mr. Groſe has favoured the public in No. XXI. the patient "was ſponged all over with cold vinegar, and directed the free uſe of yeaſt and — *wine.*" So far then as we may judge from what Mr. Groſe has told us, even he has not truſted to his favourite remedy. I doubt not, many of your readers will attribute the ſucceſs of this caſe as much to the vinegar and wine as to the yeaſt. The ſtatement of the caſe, as given by Mr. G. appears to me very vague and unſatisfactory. He tells us, that the boy was attacked on the "10th of July, with the uſual ſymptoms of typhus." On the third day, that is, on the 12th, the ſponging and uſe of yeaſt appear to have been begun. From the 13th "he became evidently better." "In two days after the yeaſt was diſcontinued." — "On the conſideration of the wonderful change which ſo rapidly ſucceeded the adminiſtration of the yeaſt," ſays Mr. G. "I cannot but imagine it to be the beſt remedy in putrid fevers ever yet diſcovered." Are we to conclude from this relation, that Mr. G's patient was cured of a putrid fever within five days from its firſt attack? If I can underſtand Mr. G. right, this is the concluſion he wiſhes to be drawn.

* See the Armenian Magazine for Auguſt 1799.

drawn. A pretty extenſive practice amongſt perſons generally afflicted with this diſorder, has never afforded me an opportunity of ſeeing a typhus fever terminate in this ſhort ſpace of time. I have ſeldom ſeen petechiæ appear earlier than the 5th, 6th, or 7th day. The criſis does not often take place earlier than the 14th or 17th day; and in the caſes which prove fatal, I have generally obſerved the fever to run on till the 21ſt day. Not that any general rule is invariably obſerved; but I have never ſeen a true typhus, with ſymptoms of putridity, finiſh its courſe in *five* days.

Mr. G. wiſhes to explain the action of the yeaſt on the living ſyſtem, from the known properties of fixed air in ſweetening putrid meat; and aſks, whether ptyaliſm is not excited as well by the external application of mercury, as by the internal uſe of it? Doubtleſs, it is; but in both caſes the effect is produced by the mercury being received into the circulating ſyſtem; and therefore the application of mercury to the ſkin, for the purpoſe of abſorption, is not analogous to the chemical combination of fixed air with putrid meat; conſequently, his reaſoning is inconcluſive.

Having now endeavoured to juſtify the ſentiments I have before obtruded on the public, I take my leave of the ſubject, having no deſign of making your Journal (which will, I am ſure, be employed in a more profitable manner) the vehicle of controverſy. I am, &c.

Kidderminster, Dec. 6, 1800. GEO. CUSTANCE.

A contagious fever, of the moſt malignant kind, appeared in London and Weſtminſter, during the months of November and December laſt. A delirium often commenced as ſoon as the third day; and a petechial eruption, though more florid than uſual, appeared on the breaſt and trunk. The other ſymptoms differed little from thoſe uſually obſerved in jail or contagious fevers.—A female patient, afflicted with this formidable diſeaſe, and with permanent delirium, was admitted into the Weſtminſter Hoſpital during the laſt month, and, as uſual, put on the generous and cordial plan; but in a few days neither food nor medicine could be adminiſtered, on account of the repugnance of the patient. In this deſperate ſituation I directed the apothecary to procure ſome freſh yeaſt, and to dilute it if neceſſary, of which a table ſpoonful was to be given every four hours. The patient took it without difficulty; it agreed with the ſtomach and bowels, and in three days an evident amendment was obſerved. The patient is now perfectly cured of the fever, and no other remedy, not even wine, was employed in conjunction with the yeaſt. T. B.

Mr. Franks, on Variolous Contagion.

[Continued from pp. 81—86 of our laſt.]

To theſe teſtimonies, the number of which might have been greatly augmented, Dr. Watkinſon obſerves, I ſhall beg leave to ſubjoin my own.— " I have paid particular attention to the point in queſtion, ſince the eſtabliſhment of the Diſpenſary for general Inoculation; and can with truth affirm, that a ſingle inſtance has not yet occurred in that charity, in which the contagion has been ſpread by an inoculated patient. Where the chance of ſpreading it has been apparently great, I have been very ſtrict in my inquiries. In many caſes the circumſtances have been ſuch, that if the apprehenſions of a celebrated Inoculator were well founded, the diſtemper muſt inevitably have been communicated.

" Some have been inoculated in narrow ſtreets, in the midſt of thoſe who were obnoxious to the Small-pox, and others in little courts, where, according to the common opinion, the danger of communicating the diſeaſe was ſtill greater.

" In the latter caſe, the patient has ſometimes been kept in a little room on the ground floor, the door of which opened directly into the court, and in the day time was ſeldom ſhut. Before this door, and within a few yards of the perſon inoculated, a number of children have continued to play during the whole courſe of the diſorder, and, as has been already affirmed, without receiving the infection.

" Baron Dimſdale indeed aſſerts, that inſtances of a contrary kind have frequently fallen within his obſervation. But, as the Baron does not ſeem to have been aware of the great influence of the epidemic conſtitution of the air—it is poſſible that what he ſhould have attributed to this cauſe, he has ſometimes imputed to ſimple contagion. Be that as it may, a writer in the *Monthly Ledger*, under the ſignature of J. S. who had an opportunity of ſeeing the practice of inoculation in the country where theſe inſtances happened, ſpeaks of the conſequences of it in a language very different from that which is held by the Baron.

" Dr. Tiſſott, in a piece intitled, *L'Inoculation juſtifiée*, very juſtly obſerves, that the Small-pox is indeed a contagious diſeaſe; but, that it does not propagate itſelf ſo much by contagion as by an *infection of the air*, produced by cauſes which are unknown to us.

" The truth of this obſervation is exemplified in a very ſtriking manner, by a fact which is related by Dr. James Sims in his *Obſervations on Epidemic Diſorders*.

" About

"About the autumnal equinox," ſays he, "bilious diſorders declined, giving way to the Small-pox, that with unheard of havock deſolated the cloſe of this year, and the ſucceeding ſpring of 1767. They had appeared above a year before, along the eaſtern coaſt of the kingdom, and proceeded ſlowly weſtward with ſo even a pace, that a curious perſon might with eaſe have computed the rate of their progreſs. In this they were ſcarcely to be interrupted, as appeared by the following inſtance. The children of ſoldiers on their march, had brought them from other places to ſome towns here, during the preceding ſummer; and although they were of a malignant kind, the afflicted all dying, and therefore moſt fit to propagate the infection, yet *not one of the inhabitants received them, until, in their regular progreſs, they had travelled over the intermediate ſpace.*

"It appears from the *Premier Rapport ſur l'Inoculation* of Dr. Petit, that the Hotel Dieu, a large hoſpital in the centre of Paris, is never free from the Small-pox, and that at certain times the ward deſtined to receive thoſe who are ſeized with that diſeaſe is extremely full; that notwithſtanding the multitude thus crowded together, and the enormous quantity of infection produced, and that in a place too which is open to the public, and where there is continually an immenſe concourſe of all ſorts of people, the diſeaſe is not obſerved to be always preſent in the neighbourhood of this hoſpital, nor even to be more common there than in other parts of the city.

"A remarkable inſtance of the inſufficiency of contagion alone to the production of this diſtemper is authenticated by Dr. Sandifort, the preſent Profeſſor of Anatomy and Surgery in the Univerſity of Leyden.

"One of the children in the Orphan-houſe at the Hague was ſeized with the Small-pox; and though the communication between the patient and the reſt of the orphans was not interrupted, none of them caught the diſeaſe.

"Another inſtance of the ſame kind, and not leſs remarkable, I remember to have heard related by the celebrated Profeſſor Van Doeveren of Leyden, in his lectures on the practice of medicine.

In the ſpring of the year 1762, a company of foot, with twelve children labouring under the Small-pox, entered the city of Groningen, which was then entirely free from that diſeaſe. Theſe children were diſperſed in the houſes of the poorer ſort of inhabitants, in the midſt of numbers who had not had the diſtemper, and who, conſtrained by their poverty, could not fly from its approach.—A fairer trial of the power of ſimple contagion could ſcarcely have been deviſed. The event

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was ſuch as convinced the learned Profeſſor that this power was inadequate to the effects which had been commonly aſcribed to it.—The epidemic threatened was not produced, nor, which is more extraordinary, was the diſorder propagated ſporadically; for, after the moſt ſedulous inquiry, not an individual could be found to whom the infection had been communicated.

"To multiply facts of this ſort, Dr. Watkinſon obſerves, would be eaſy, but I truſt that theſe will be quite ſufficient to eſtabliſh the poſition for which they were brought. The Doctor therefore quits this ground, and meets the Anti-Inoculiſts upon that in which they have hitherto thought themſelves ſecure of victory, namely the Bills of Mortality. And in this reſpect the Doctor appears to be no leſs happy, for he has ſhewn, that although the prevalence of inoculation and the increaſed mortality of the Small-pox, may at ſome particular periods have coincided, (for according to the laws of chance, this muſt ſometimes have happened) yet, from the great irregularity of their coincidence, it may be fairly inferred, that *they are not connected together as cauſe and effect*. And upon this fact, the Dr. obſerves, "the defence of inoculation againſt the charge of increaſing the mortality of the Small-pox, might perhaps be ſafely reſted; but I ſhall add to it another, derived likewiſe from the Bills of Mortality, which appears to be concluſive. For the fact alluded to, I am indebted to my ingenious friend Dr. James Sims.

"An objection has been made to inoculation, and lately ſupported with conſiderable warmth by ſeveral very reſpectable writers, which, if founded in truth, would be ſufficient to prove that the practice of this art is detrimental to ſociety. It is aſſerted, that, by inoculation the contagion is ſo much propagated, that the victims to the Small-pox have been more numerous ſince than they were before that practice obtained; and that the mortality has increaſed in proportion to the reception of the art.

"To prove theſe aſſertions, it has been uſual to extract the deaths by the Small-pox from the Bills of Mortality, for a certain number of years previous to the introduction of inoculation; and to compare the general average of theſe with the average of deaths ſince that time; and by dividing the latter into ſeparate periods of years, to ſhew, that the proportion of deaths by the Small-pox has been conſtantly increaſing ſince the practice of inoculation began.

"Thus, one writer, who gives a view of the Bills of Mortality for eighty-four years, ſhews, that in forty-two years previous to inoculation, only ſeventy-two deaths in every thouſand were owing to the Small-pox; whereas, in forty-two years afterwards,

afterwards the deaths by that diſeaſe amounted to eighty-nine in every thouſand; and that, by dividing the latter of theſe into leſſer periods, the average of deaths is as follows:—In the firſt twelve years it is ſeventy-four in a thouſand; in the next ten, eighty-three; in the next, ninety-ſix; and in the laſt ten, one hundred and nine.

"This conſtant increaſe is attributed to inoculation; and the argument appears to be properly ſtated, as it guards againſt any deception which might ariſe from the variations in the general number of deaths: I have endeavoured to ſtate this objection in the ſtrongeſt manner, and hope that I ſhall be able to give a deciſive anſwer to it.

"The reaſon why the above objection has not hitherto been ſatisfactorily anſwered, is this; theſe who have attempted it, have taken the Bills of Mortality as garbled and unfairly ſtated by the objectors to inoculation, without giving themſelves the trouble of further examination.

"The circumſtance in which the objectors have dealt unfairly by us is, that in taking the medium of deaths for a certain number of years prior to the practice of inoculation, as a fixed ſtandard, they have not once hinted that the mortality of the Small-pox had increaſed in the ſame proportion before, as it has done ſince the introduction of that art; and by prudently publiſhing only a part of the Bills, they have given us no opportunity of making this diſcovery. Had they given the whole of the Bills, is it to be ſuppoſed that any man in his ſenſes would have joined with them in blaming inoculation for an increaſe, *which commenced ninety years before inoculation was heard of in this country, and continued progreſſive through the whole of that period?*" Dr. Sims has conſtructed eight tables; theſe tables will ſhew that the mortality from caſual Small-pox, inſtead of increaſing, is at preſent (*id. eſt.* more than twenty years ago) conſiderably declining.

Thus have I extracted from Dr. Watkinſon's work, a body of evidence in ſupport of the opinion which I ſubmitted to the readers of the Medical and Phyſical Journal in a former number, namely, that the ſuperiority of the Vaccine over the Variolous Inoculation is ſomewhat over rated; by ſhewing that the accuſation which has been brought againſt inoculated Small-pox, of propagating contagion, and increaſing the mortality of caſual Small-pox, is void of foundation.

Had it not been for this unfortunate objection, the inſtitution for inoculating the poor of London, at their own habitations, would have continued to exert its benign influence, and ere this have reſcued thouſands, who may have fell victims to the malignity of Small-pox; but it was repreſented as fraught with

with very dangerous consequences to the community, and is tending, by spreading the contagion, to increase the very evil it was designed to lessen.

Until a sufficient number of similar institutions are established in London, to inoculate the whole of the poor with vaccine or variolous matter, as each individual may prefer, let the Ministers in every parish be enjoined, by proclamation if expedient, to take opportunities occasionally to recommend the inoculations for universal adoption, by pointing out to their audiences the incalculable benefits which would arise from such a procedure. Let these recommendations from the pulpit be seconded by the governors of the poor;—let it not become a parish job;—let not the poor's rate be increased on this account;—let not the concern be entrusted to one or two individuals in a parish; but let every medical person resident in the parish be invited to lend his assistance towards inoculating the whole of the poor at their own habitations, for Small-pox or Cow-pox.

In a matter of such importance, none, as good men and good subjects, would refuse; on the contrary, a degree of emulation would be excited, highly beneficial; all would be anxious to participate in so glorious a work: Thus would the cause of humanity be served, and the best interests of the country promoted, by adding to the population of it. An estimate may be formed of the immense number of lives which would be yearly saved, were the *variolous inoculation only employed*; when we recollect that Baron Dimsdale, whose practice must have been very extensive, has informed us, *that he did not lose a single patient in the course of twenty years from inoculation.*

At the end of every year, each individual should be requested by the vestries to deliver in an account of the number inoculated, with names and places of abode, within the year, and for which they have received no sort of remuneration: These accounts should be published for the information of the parishioners, and a subscription might then be proposed by the vestries, which would enable them to tender to their professional brethren, something more than the thanks of the parish for their laudable undertakings.

If something like this plan should be adopted, instead of between two and three thousand who die annually in London of Small-pox, the mortality of this disease would be reduced to a cypher. I am, &c.

Smith Street, Westminster,
Dec. 10, 1800.

JOHN FRANKS.

To-

To the Editors of the Medical and Physical Journal.

GENTLEMEN,

THE efficacy of Inoculation of the Matter of Cow-pox, in rendering the human body unsusceptible of Small-pox, *appears* now to be fully and completely established; at least, *as far as we can conclude from the experiments* hitherto made, and the time that has elapsed since their institution.

Every generous mind will congratulate himself on a discovery that promises so much benefit to mankind, and will experience the highest pleasure from the successful labours of those who have so ardently exerted themselves in the cause of humanity and science.

I feel myself deeply interested in the success of this Inoculation; and it is with reluctance I here take the liberty of submitting a Case, that seems to militate against the permanent preventive influence of the Variolæ Vaccinæ.

This is the grand desideratum, the determination of which can be effected only by length of time. Three weeks since, I was requested to visit Mrs. Dredge, aged 55 years, living in the neighbourhood of Castle-Cary, whom I found labouring under Small-pox of the distinct sort, which she caught from a servant-boy living in her house. She informed me, that she did not expect this disease, since she had taken, twenty-eight years before, the Cow-pox from milking cows affected with the same. She observed her disease was very severe, having had numerous pustules on the hands and fingers; lost two nails; had a considerable tumour in the axilla, and great pyrexia. Sixteen years ago, she was much exposed to Small-pox in her own family, having children ill of it, both naturally and from inoculation, and whom she constantly attended, but escaped infection. She says, that she has not been subsequently exposed to it till the time abovementioned. Her sister was affected with Cow-pox at the same time, and from the same cows; had a large tumour in the axilla, several pustules, and much pyrexia. Twenty years afterwards this woman attended her own children in the natural Small-pox, (one of whom died) but without receiving any infection. The disease among the cows extended through the whole dairy, consisting of about twenty cows; and the milk was so much affected, as to be rendered unfit for use for some time.

The above History may be considered as an addition and illustration of the two Cases communicated by Dr. Sims, in the first and second volumes of the Medical and Physical Journal.

Journal. It will probably be said, that, from the large and decisive experience of the infallibility of the Cow-pox virus in preventing Small-pox, little credit or confidence can or ought to be given to the few and almost solitary opposing instances now adduced, and especially as some dubiety remains of the genuine nature of the disease. I admit the propriety and force of this reasoning, but cannot at the same time, withhold this information from the public eye. I am, &c.

JAMES WOODFORDE, M.D.

Castle-Cary, Dec. 5, 1800.

To the Editors of the Medical and Physical Journal.

GENTLEMEN,

CONVINCED that no preliminary Dissertation is necessary to invite your attention, or that of the whole medical world, to any circumstance which will tend to promote the very general and humane attempts to introduce the Cow-pox: I shall presume to make an observation or two, which I venture to believe will tend in some degree to promote the grand, benevolent design. In the practice of medicine, generally considered, it is impossible that practitioners can ever devote too much attention to all the minor parts of the subject which calls for their consideration; a want of this care has too frequently defeated the best laid plans, and the almost apparent certainty of success: This observation applies more particularly to the attempts at introducing a new disease; every little circumstance attached to it is of the highest importance, and every endeavour must be used to combat general as well as local prejudices, to render its good effects as manifest as possible. The common practice of conveying the variolous matter from one town or village to another, when Inoculation for the Small-pox became pretty general, was by means of threads imbued with the matter, enclosed in paper; a pretty free incision was made in each arm to receive the thread, in order that infection might be certain: But when it was found that Small-pox matter was of a very virulent nature, and that portions of it, when detached in this way from the body of a sufferer, retained their poisonous nature for a considerable time, Operators contented themselves with barely moistening the point of a lancet or needle with it, for use on some distant day; a method which but rarely failed, where every other circumstance was favorable for

for the reception of the disease, and attention was paid to the exact point of time when the matter was fittest for being used. Not so the Cow-pox matter, I apprehend; I am convinced more attention is necessary on this head, than has yet been given to the subject. The difference in degree of malignity, or strength of the two fluids, seems now sufficiently obvious, and it will be instantly allowed, that we cannot too forcibly impress this truth on the minds of all who will give us a candid hearing. When this point is obtained, and any friend desires to submit their child to the operation, it is one of the first wishes of both parties, that the disease may succeed after the first time of inserting the matter: When this desirable event fails to take place, the disappointment is great; and if it occurs often, will tend much to retard the progress of the highly important business.

The first child I was desired to inoculate with Vaccine matter, resided in the same house with an intelligent Physician of this city, who took great pains to procure proper recent matter; it was taken on a lancet in the usual way, and the day following I used it, making a very free scratch in each arm, so as to draw blood; which mixing with the gummy matter of the pox, formed a kind of scab on each little sore: This remained on some days, but produced no infection. At the end of sixteen or eighteen days, a fresh supply of matter being obtained, I made the incisions larger, carefully moistened the dried fluid with steam, &c.; but to as little purpose as before. Notwithstanding these disappointments, and the subject being a delicate child under six weeks old, the mother cheerfully requesting a third trial, my patient was carried to a house where two children were under the disease, and inoculated with limpid matter warm from the arm of one of them; this succeeded, and the disease run its ordinary safe course: In the interim I received matter from London and Gloucestershire, on thread, inclosed in paper, neither of which, though used with every care, produced any effect. As before remarked, considerable chagrin follows the failure of infection from the first operation, even in Small-pox; some parents I have known, who would have deemed it presumptuous to permit even a second trial. The part of the country I live in, although, perhaps, the first milk county in the kingdom, affords little or no opportunities of collecting the original Cow-pox virus; we have, therefore, hitherto had to procure it from a distance. To prevent others experiencing the mortification of want of success the first time of operating, is my intention in proposing the method of taking and preserving the variolous virus, taught me by my first preceptor, the honoured and ingenious Mr. James Wyke,

 surgeon,

ſurgeon, of Colebrook-dale, to be adopted for the more ef-fectual preſervation of the vaccine.

Procure a ſmall phial, the ſmaller the better; fit it with a tight cork, having previouſly expelled the damp air from it by placing it near the fire a ſhort time; take a thread of cotton yarn, about an inch, or inch and a half long, make a puncture in the veſicle, preſs the centre, or middle part of the cotton when held like a loop, thereon, at the ſame time untwiſting it a little, thus imbibing all the moiſture you can from the part; twiſt the ends together, and place in your bottle, ſo as not to touch its bottom, the cork of which will keep it tightly againſt the neck; put it into a warm pocket, and carry it about with you until wanted; when required to be ſent to a diſtance, though a bottle be not a convenient article to tranſmit by letter, yet we have now the comfort of ſuch a number of other conveyances, that this obſtacle ſinks into nothing, when compared with the ſatisfaction ariſing from ſucceſs. When about to be uſed, if the phial be immerſed in hot water a ſhort time before uncorking, the matter will rarely be found dry and brittle, as it almoſt always is on a lancet; and if it ſhould be ſo in a ſmall degree, ſtill it will contain the active principle unevaporated, whilſt that on the inſtrument is too often wholly inert. Since writing the above, I perceive by a York Newſpaper, Dr. Cappe mentions the advantage reſulting from keeping the vaccine matter on glaſs, and that from this ſubſtance it can ſuffer no injurious change. Though this be an undoubted fact, as to the matter of glaſs itſelf, yet, I apprehend it will not ſecure the fluid from the influence of the air, which I imagine will be allowed to be the efficient cauſe of injuring its activity in any other way than incloſure in a phial. I need not dwell a moment on the property cotton ſeems to have of imbibing and retaining contagious miaſma. Mr. Simpſon of Malton, Yorkshire, recommends a ſlip of glaſs with the matter *dried* on it, to be run through a cork, and then incloſed in a ſuitable bottle; this mode comes neareſt the one propoſed, but I imagine the cotton will be found preferable, on account of its *not* ſuffering the matter to dry ſo ſpeedily as glaſs. Having written thus far, your laſt Journal came to hand; I find therein a letter from Dr. Marſhall, at Gibraltar, to Mr. Ring, near the concluſion of which, he ſays, "Some of the matter we uſed for inoculation here, was what you obligingly furniſhed us with, and we find it perfectly efficacious, although no precautions had been uſed as to the preſerving it, more than putting it in a *ſmall phial*.

Again, Dr. Waterhouſe, of Cambridge, New England, writes thanks to his friend in Bath for the *phial* of vaccine

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matter, and ſays, "its activity I have experienced on my neareſt connections:" I have no doubt the learned Profeſſor was well pleaſed at his ſucceſs, but I have every reaſon to believe, had he received an infected lancet, or bit of thread in paper, he would, on the other hand, have been much diſappointed. Before concluding my letter, permit me to add an obſervation on the means of inducing the poorer claſſes of the community to avail themſelves of this invaluable gift of Providence, for, unleſs we can cauſe *them* to liſten to our plan, unleſs we can fully arreſt *their* attention, our progreſs, I fear, will be but ſlow; on the contrary, this deſirable object once attained, the buſineſs will be complete. All the medical gentlemen of this city have zealouſly come forward with a tender of their ſer-ſervices to inoculate the poor, gratis, whenever applied to for that purpoſe; this is a great ſtep, but ſomething more muſt be done; their underſtandings muſt be convinced of the ſafe, ſalutary, and certain conſequences of the operation, before they will apply to us to ſerve them. Would not the printing, on a ſuitable paper, a few of the cleareſt, moſt ſtriking facts, in the plaineſt language, and freely circulating them amongſt individuals of this deſcription, joined with the powerful force of example in the higher orders, have this effect? I am, &c.

Cheſter, Nov. 17, 1800. G. N. HILL.

To Dr. BATTY.

Dear Sir,

THE active and unremitting exertions of the Editors of the Medical Journal to diffuſe a general knowledge of the great advantages which reſult from the Jennerian Inoculation will, I have no doubt, enſure inſertion in your next Number to the encloſed additional Teſtimonial, ſigned by nearly all the phyſicians and ſurgeons in theſe large towns. You will be much gratified to hear, that the ingenious phyſician of the fleet, Dr. Trotter, (than whom, I believe, there is hardly a man more alive to the extenſion of medical improvements) and the navy ſurgeons, are procuring an elegant and very appropriate GOLD MEDAL, to be preſented to Dr. Jenner; an honourable and generous reſolution, expreſſive at once of their great humanity, of their zeal for the ſervice, and of their reſpect for, and attachment to ſcience! Indeed it is very true, and I feel great pleaſure in making the remark, that on this great occaſion, as well

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as in the profecution of many other late interefting inquiries, thofe gentlemen have evinced an ardour of purfuit, which certainly reflects on them great reputation, and will, it may be hoped, bye and bye, (when it fhall be recollected how expenfive their claffical education, in the greater number of inftances muft neceffarily be, how much further expence, and how many years of indifpenfable and folicitous application, the nature of their fubfequent ftudies always requires) fecure to this truly refpectable and important clafs of officers, a larger fhare of public remuneration. I am, &c.

Plymouth Dock, Jan. 10, 1801. R. DUNNING.

For the MEDICAL and PHYSICAL JOURNAL.

THE introduction of the Vaccine Inoculation in the populous towns of Plymouth and Plymouth Dock, at a very early period after the commencement of the practice in this country, and the continuance of it on an extenfive fcale to the prefent time, have afforded fufficient opportunities to a numerous body of practitioners to confirm by their own obfervations, and by the teftimony of their immediate friends, the ample and refpectable evidence which has been adduced in favour of this eminently ufeful difcovery. The general fuccefs of the new Inolation has not, however, prevented the circulation of reports of unfavourable accidents, and of occurrences inconfiftent with its character of abfolute fafety in itfelf, and of affording permanent fecurity againft the future invafion of Small-pox. As the prevalence of fuch reports muft have a tendency to excite doubts of the fafety and efficacy of the new Inoculation in the minds of thofe who have not the opportunity of tracing them to their origin, and who are not acquainted with the ftrength and folidity of the mafs of evidence on which the fupporters of the new practice ground their conviction of its utility, we have thought, that it might not be unattended with advantage to the public, at leaft to that fmall part of it to which our names and characters are known, to add to the numerous atteftations already given, a Declaration, as well of our conviction of the efficacy and fecurity of the practice of inoculating the Cow-pox in general, as of our full perfuafion, refulting from an inquiry into the circumftances of the cafes adduced, that no inftance has occurred in this neighbourhood, which is entitled in any degree to call in queftion the reafonablenefs of this conviction, the reports which have arifen being evidently founded in error and mifreprefentation. The effects of mifreprefentation can be obviated only as they arife; but it may not be amifs to obferve here more particularly, that the Small-pox has lately prevailed

prevailed in this neighbourhood to a confiderable degree, and that, in all the inftances in which this difeafe is faid to have occurred after inoculation for the Cow-pox, the infection of the former was evidently received before the inoculation for the latter, or the operation had failed of producing thofe fymptoms of conftitutional affection, on which alone fecurity depends. The fources of error will, we truft, be in future avoided, by due attention to thofe cautions in the practice, which have been fo fully pointed out by Dr. Jenner and others.

Plymouth and Plymouth Dock, Jan. 7, 1801.

R. B. Remmett, M. D.
William Woollcombe, M. D.
T. Trotter, M. D.
R. Sheppard, M. D.
James Gafking, M. D.
Robert Fortefcue,
James Courtenay,
George Cooban,
Richard Dunning,
D. Little,
Vaughan May,
Robert Sargent,
Andrew H. Sargent,
Thomas Stewart,
John Smith,
D. W. Spry,
J. Lower,
John Penkivil,
J. Rivers Lancafter,
Samuel Fuge,
Thomas Thynne Folds,
Stephen Hammick,
Stephen Hammick, jun.
Nathaniel Seecombe,
Thomas Richardfon,
Richard Jones,
J. Beard Samhill,
R. H. Armftrong,
W. Bragg,
Peter Rowe,
John Bone.

Profeffor De Carro's Letter to Dr. Pearson.

Dear Sir,

I CANNOT wait any longer to communicate to you my further fucceffes in the Vaccine-pock Inoculation. A dreadful Small-pox epidemic has given, this autumn, a new luftre to that practice which, according to the fenfation that it is making in this town, is not likely to be ever laid afide.

This extenfion of practice, which hitherto is entirely, or nearly fo, in my hands, has enabled me to eftimate *the accuracy of your obfervations on the inefficacy of a Cow-pock that comes too rapidly, and which does not follow the laws of that difeafe.* I muft candidly confefs to you, that till this autumn I had not a very exact idea of the Vaccine puftule; that is to fay, I knew very well how it fhould have been, to be fimilar to the defcription of the Englifh Inoculators, fuch as I had feen it on my own children: but I was not aware that it muft be abfolutely

abſolutely ſo, and that every appearance after inoculation was not to be looked upon as a true Vaccina. The aſtoniſhing likeneſs of my Vaccine puſtules in my ſixty laſt inoculations, has given me a true knowledge of it; and I ſcarcely believe that I could be in future miſtaken. On the third or fourth day one perceives a ſlight elevation and redneſs; it increaſes in a veſicular form till the twelfth or thirteenth day, and contains always the moſt limpid fluid; a fever comes on the eighth or ninth day, or ſometimes is not perceptible; the beautiful areola appears on the ſeventh day, and increaſes till the cruſt is entirely formed; this cruſt begins, commonly, on the twelfth, and becomes quite black.

I have had ſome caſes originating from Count Mottet's matter, where a patient has received the Small-pox by inoculation, three by natural contagion, and one has retaken the Vaccina in its perfect form. Luckily for the ſucceſs of the inoculation, the parents of theſe children were adviſed previouſly of the idea I had of the imperfection of their former Vaccina, and they were not aſtoniſhed at it, after having had a great difficulty in believing that the ſuperficial cruſt it had formed would not be ſufficient to preſerve them from future infection.

I have had two caſes where the children were already infected with the Small-pox at the time of the Vaccine inoculation, and where it ſhewed itſelf as uſually, without interrupting, in any degree, the courſe of the Cow-pock. One of them proved exceſſively mild, the other is ſtill dubious at the time I am writing.

I have had a Caſe where the inoculated ſpot gave ſigns of inflammation only on the twelfth day, and where the puſtule has been perfectly regular and complete. It was the ſame ſubject that had been, ſeven months ago, inoculated wlth the imperfect Cow-pock.

I have not yet ſeen any puſtules, either of the *fugitive* or of the *variolous-like* appearance, although, according to Doctor Woodville's obſervation, they ſhould appear more commonly where the Small-pox is epidemic: There never was, perhaps, ſuch a diſaſtrous epidemic at Vienna, as that we have now. It is certainly owing to it that people think ſo much now of the Cow-pock, againſt which they have been exceedingly incredulous. I hope to have it in my power to continue it all the winter.

By the laſt accounts I have of the progreſſes of the Vaccine Inoculation at Geneva, they are exceedingly rapid. They have lately obſerved ſome eruptive Caſes; but I need not enter into theſe particulars, as you will have them immediately from Drs. Pechier and Odier, or from the *Bibliotheque Britannique*.

I have

I have had, lately, two opportunities to make experiments upon persons who have *undoubtedly* had the Small-pox in their infancy; they are both medical men, and consequently their testimony is more admissible than that of most men. *Upon one, the Vaccine matter produced no effect whatever; and upon the other, a very small, superficial vesicle appeared, which soon changed into a crust.* In one word, there was not the least appearance of the true and usual Cow-pox pustule. *They confirm your principle, that one cannot have the Cow-pock after the Small-pox.*

One single circumstance vexes me in my Vaccine practice: It is the frequency of inoculations that fail; that is to say, where no pustule nor inflammation is produced. I can scarcely attribute it to my method, which is the common puncture of most practitioners. I make it as superficial as I can, and avoid as much as possible to draw blood, which, I believe, can wash off the matter. I make one puncture on each arm.

I see that, in England as well as abroad, Inoculators are divided upon the advantages of the puncture or of the incision: You and Dr. Jenner seem to make use of the puncture; Dr. Woodville recommends strongly the incision. At Geneva they prefer the incision; and here, the puncture. I propose now to make comparative trials, and inoculate my first twenty patients at one arm, with the puncture; at the other with the incision. The difficulty of making the most superficial incision, without drawing blood, has made me hitherto adverse to that method. Upon the whole, I dislike much to be so often obliged to repeat the inoculation. You will oblige me very much to enter with me into some particulars upon this point.

My second patient, where the two diseases have gone on together, has had also a very favorable Small-pox. Do similar Cases in England authorize you to believe that the Cow-pock can render the Small-pox milder, when they attack the same subject together?

The medical world is waiting with impatience for the Work you have promised in your *Statement*, &c. I join my intreaties to those that must already have been addressed to you, upon the publication of a Work which must throw the greatest light on a subject that engages now the attention of all civilized nations. Whenever my materials shall be sufficient, I propose writing a complete Treatise on the subject, for the use of the Continental Physicians, and such persons who are not in the way of collecting from the various torrent of information, which a little zeal and activity have opened to me. I have the honor to be, DEAR SIR,

With the sincerest esteem, &c.

Vienna, Oct. 22, 1800. J. DE CARRO.

P. S. I sent lately some Vaccine matter to Lord Elgin, at Constantinople, for the inoculation of his only son.

J. BRANSON'S, Efq. Surgeon, Letter to Dr. PEARSON.

DEAR SIR,

IN my laft letter to you, I mentioned my ideas refpecting the infufceptibility to the Vaccine Contagion in a perfon who had previoufly had the Small-pox. I am ftill of the fame opinion, *as I have never yet been able to produce the true vaccine puftule in a perfon who had before undergone the Small-pox.* But I have lately had a Cafe, from which it would appear, that if both difeafes are introduced into the fyftem about the fame time, that they will both be received, and proceed through their regular ftages, without being at all affected by each other. — A child was inoculated for the Cow-pock after it had been for fome days expofed to the contagion of the natural Small-pox, on Friday, the 26th of September. The arm went on in the ufual way, until the Wednefday evening following, when the child was feized with fever; and in the evening of the next day the Small-pox appeared. The eruption was confluent, and particularly fo about the inoculated part. On Monday the 6th of October, which was the eleventh day from the infertion of the Vaccine matter, and the fifth from the commencement of the fever of the Small-pox, I inoculated a child from the Cow-pock puftule, furrounded by a clufter of Small-pox; in fhort, they appeared fo blended, that I had my doubts whether the matter I obtained was that of the Cow-pock, or mixed with the Small-pox: Indeed, I was rather difpofed to think the latter was the cafe, as it did not appear fo limpid as it ufually does in the Cow-pock. However, the refult was, that it produced the genuine Cow-pock, without any appearances different from what I have always feen; and with matter from this fource I have continued to inoculate ever fince, without any variation either in fymptoms or appearance. If this Cafe furnifhes you with any thing new on the fubject, I fhall be very glad: I have no time now to make any comments upon it, as the Poft-office is juft going to fhut. I am,

DEAR SIR,

Your's, fincerely,

Doncafter, Nov. 23, 1800. J. BRANSON.

P. S. Since I wrote laft to you, I have inoculated upwards of five hundred for the Vaccine Difeafe, without any unpleafant fymptom.

To the Editors of the Medical and Phyſical Journal.

GENTLEMEN,

I Should be ſorry to excite prejudices in the minds of your readers, or of the public, againſt the introduction of the Vaccine Diſeaſe as a ſubſtitute for the Variolous, being well convinced that, in many reſpects, much preference is due to it, and that great credit attaches to thoſe gentlemen who have ſo meritoriouſly ſtepped forward in its ſupport; but I have to lament, that its advantages are ſomewhat over-rated, when we are told, from reſpectable authority, that it is a diſeaſe free from danger, and that, when introduced by inoculation, the ſymptoms in children are uniformly mild, from the inſertion of the matter to the period of ſcabbing. It is a great misfortune, that proſelytes to new ſyſtems do not always carefully examine into opinions handed down from high authority, but give them implicit credit; and it is equally unfortunate, that, in the recommendation of any new doctrine, the unfavourable ſymptoms are too frequently placed in the back ground, and only the more pleaſing ones expoſed to public view.

Occaſionally we hear of patients inoculated with Vaccine matter who have had the diſeaſe with ſome ſeverity, but we are not told in what the violence of it conſiſted.

In the two caſes I ſhall take the liberty of laying before the public, there are ſome circumſtances which are by no means explicable by any account I have yet heard of the diſeaſe; and though ſymptoms ſomewhat ſimilar are recorded in No. XXII. of your uſeful Medical Journal, yet, in many reſpects, the facts will be found to vary much.

In thoſe caſes the ſymptoms were extenſive eryſipelas, ſpreading rapidly from the inoculated part, accompanied by conſiderable conſtitutional affection. Theſe caſes were ſubmitted to the inſpection of Drs. Wollaſton, Pearſon, Griffiths, and Crofts, whoſe opinions were, that the unfavourable ſymptoms aroſe from the matter with which the children were inoculated having been taken at a late period of the diſeaſe, after a brown ſcab had formed, and when it had acquired a purulent appearance, and conſequently had, by degenerating, loſt the power of giving the Cow-pox infection. This opinion was alſo ſanctioned by Dr. Lettſom, who ſaw the patients.

I know many inſtances where Vaccine matter has been taken from the arms of patients as late as the eleventh and twelfth days, when it had acquired a purulent appearance, and a brown ſcab had formed, which matter has been inſerted into the arms of

of children, on whom it has produced a very mild diſeaſe, and from whom fluid matter has been taken to inoculate others, who have alſo had the diſeaſe in a manner equally mild. It will be difficult to reconcile theſe facts, which are atteſted not only by my own experience, but by that of ſome very reſpectable ſurgeons in this place, with the opinions advanced, that matter taken at ſo late a period loſes, by degenerating, the power of producing the diſeaſe.

On the 22d of October, 1800, I inoculated Mr. Hulſe's child, aged ſix months, with fluid matter taken on the eighth day from the arm of a child who had the diſeaſe very favourably. Until the eighth day, appearances were promiſing, and indicated a mild diſeaſe; but on the ninth, the parts ſurrounding the inciſion began to inflame, and became very hard: The inoculated part, inſtead of putting on a puſtular appearance, degenerated into an ulcer, and at this time much derangement of the ſyſtem took place, as fever, ſickneſs, and great inquietude.—I applied emollient cataplaſms to the arm, and dreſſed the ſore with equal parts of ungt. hydrarg. nitrat. and ungt. ceræ, notwithſtanding the uſe of which, and warm fomentations, &c. the parts changed their appearance very little until the ſixteenth day, when the inflammation extended to the ſhoulder and hand, on the fingers of which, ſeveral veſications appeared. While theſe parts became the ſeat of diſeaſe, the inflammation completely left the inoculated part, and the ulcer acquired a healthy aſpect. As the inflammation left the hand and ſhoulders, it ſpread to the back, breaſt, and face, on the ſame ſide, and after remaining there about twenty-four hours, it diſappeared, leaving the parts very much diſcoloured, and attacked the other ſide of the back and breaſt, and afterwards the arm and hand; but before it became ſo violent as on the ſide firſt affected, the child was exhauſted by want of reſt and exceſſive irritation, and died on the twenty-ſixth day after inoculation. It is right to ſtate, that on the twelfth day, the nurſe who ſuckled this child, was taken ill of an inflammatory rheumatiſm, and continued ſo during the time it lived; and as it refuſed almoſt every other kind of food but the breaſt milk, I fear its ſymptoms might be increaſed, by the neceſſity we were under of allowing it the breaſt to keep it quiet and give it nouriſhment. Not having ſeen or heard of any caſe ſimilar to this, I was willing to attribute the ſymptoms to accidental cauſes; but, a few weeks afterwards, another caſe occurred, which, from its near reſemblance to this, convinced *me* they were the conſequence of the Cow-pox.

On the 27th of November, I inoculated in both arms a very healthy child of Mr. Green's, of Lenton Abbey, aged five

five months, with fluid taken on the ninth day from the arm of a child equally healthy. Previous to the eighth day there was no appearance which indicated any thing unfavourable. The incisions in each arm were gradually elevated into small pustules, filled with limpid matter, with a small inflammation round their base. On the ninth day the inflammation on the left arm spread very rapidly, and before the next day it occupied the whole arm, but without any vesications, as in the preceding case By the twelfth day the inflammation was much diminished on the arm, but appeared on the back, breast, and face on the same side. At this time the incision on the left arm, which, on the tenth day, had degenerated into a sordid ulcer, became almost dry, and began to form a scab, the inflammation surrounding it being intirely gone. After the inflammation had continued on these parts about twenty-four hours, it removed to the right arm, which was now dry, and had scabbed over.

It extended from the shoulder to the elbow, and also affected the right side of the body in a similar manner it had the left, and after having exercised a kind of sportive fancy on different parts of the body, for at least five weeks, a small abscess formed in the axilla, on the right side, and the child perfectly recovered. The incisions in both the childrens' arms were made very superficial and small, and the matter was used a few hours after it was taken, being first diluted with a little warm water.—Mr. Green's child was at its mother's breast, and had it not experienced the most unremitting attention, I have much reason to fear, the consequences might have proved serious. From this child's right arm I took some matter on the ninth day, with which I inoculated two children, each of whom passed through the disease as favourably as any I ever saw.

These facts are most impartially related, and no circumstance is omitted which I thought worthy attention. I believe, after all, we must refer much to idiosyncrasy; and as it frequently occurs in the variolous disease, that two children inoculated with the same matter, and apparently under similar circumstances, shall be very differently affected, so it will occasionally happen to those inoculated with Vaccine matter. I am, &c.

Nottingham, Jan. 8, 1801. B. MADDOCK.

To Dr. BATTY.

My dear Sir,

YOU will excuse my troubling you with this note, concerning an Error which has been committed in the printing off the sheets of my Communications on Diabetes, in the last Number of the Medical Journal. I find that the pages have not been printed off in the order in which they were written, so that in the perusal a great want of method and arrangement must necessarily arise. This being the case, I have to ask it as a favor of you and your Colleagues, that you would print in a conspicuous part of the next Number of your Journal, (and not among the Errata) an address to your Readers, requesting them, in looking over my Remarks, to insert the paragraph, at the foot of the 66th page, beginning with the words, "And in treating of the extractive matter," *together with all the following paragraphs immediately before the paragraph in page 62, beginning with the words,* "But in ſpeaking upon this ſubject;" *by which insertion the order will be restored, and the Error corrected. I am, &c.*

Norwich, Jan. 1, 1801. *R. LUBBOCK.*

To the Editors of the Medical and Phyſical Journal.

Gentlemen,

ABOUT ſeven o'clock on Wedneſday evening, (Dec. 31) a hurrying meſſage was brought, requiring Mr. Chamberlaine's attendance to a young man who had ſwallowed, with an intention of putting an end to himſelf, a quantity of laudanum.

Mr. Chamberlaine being at that time at ſome diſtance, I offered my ſervices, which were accepted; and knowing no time muſt be loſt, I took with me ℨſs. of ipecacuanha, and ſix grains of tartar emetic. I found the young man in a ſtate of inſenſibility, the pupils of the eyes widely dilated; pulſe irregular, and ſcarcely perceptible; ſubſultus tendinum; ſtertor; cold ſweats, and every bad ſymptom.

A phial which, when full, might contain an ounce and a half of laudanum, was produced. On inquiry, I learned that he had brought it home quite full; he had taken the largeſt half, or rather more than ſix drachms: and from every circumſtance that could be collected, a ſpace of time not leſs than fifteen, or perhaps, even twenty minutes, muſt have elapſed before it was diſcovered that he had taken it; and ſome minutes more

elapſed,

elapſed, of courſe, while aſſiſtance was ſought after. The emetic I brought with me I mixed in ſome warm water, and his mouth being opened with ſome difficulty, it was poured in, but finding that this produced only two convulſive motions of the ſtomach, I haſtened home, (ordering the people with him to keep him in continual motion, and on no account whatever to ſuffer ſleep to come on) and returned with two papers, the one containing ℈ſs. and the other ℈j. of zincum vitriolatum, with a ſmall quantity of tartar emetic: I gave him the former one directly, and the latter in about four minutes after, drenching him with great quantities of warm water. This rouzed him a little; but ſtill, as it only vomitted him twice, and that not in ſuch a manner as I could depend upon for the preſervation of his life, I again went home, and returned with two papers containing ℈j. in each. I now found convulſions and ſtupor coming on very quick, in a greater degree than at firſt, with his teeth claſped, and pulſe ſtill more weak and irregular; I forced open his mouth, and poured down the ſolution of zincum vitriolatum, continuing to give him plenty of warm water, and followed up this quantity with the ſecond; in about five minutes theſe vomitted him twice very well, and on my repeating the warm water, in which I diſſolved ℈ſs. of zincum vitriolatum, they vomitted him in ſuch manner, as to put him out of all DANGER. The convulſive motions of the upper extremities now began to go off by degrees, and every other dangerous and alarming ſymptom, peculiar to this narcotic drug, to ſubſide. I ordered him to be walked about the room at intervals, and to be kept awake for upwards of ſix hours. He felt his ſtomach and throat very rigid and ſore the enſuing morning, but when I went in the evening, accompanied by Mr. Chamberlaine, he was perfectly recovered; and on viſiting him ſince then, found him perfectly well and very thankful. I am, &c.

CHARLES COLLIER.

Ayleſbury Street, Clerkenwell,
Jan. 5, 1801.

To the Editors of the Medical and Phyſical Journal.

GENTLEMEN,

THE laſt Number of your very uſeful Journal contains an intemperate letter, addreſſed by Dr. Ferriar, of this town, to Dr. Batty, in which the Doctor attempts to vindicate himſelf againſt two charges, publiſhed in my Eſſay on Phlegmatia Dolens,

lens, namely, of *borrowing* his account of the theory and cure of this diſeaſe from Mr. Trye's *Essay on the swelling of the lower extremities incident to lying-in women*, and of *misrepresenting* the caſe of Jane Waters.

The remarks I ſhall now make upon the Doctor's letter, will, I doubt not, convince your numerous readers, that both the charges are well founded, and that the Doctor would have acted more prudently, if he had not publiſhed his vindication.

Dr. Ferriar declares, that he derived his opinion of the nature of the diſeaſe from the evidence of his ſenſes, without obligation to any preceding writer, and that his method of cure was directly deduced from that opinion: He further declares, that he has never *read* Mr. Trye's publication, which I have mentioned as the ſource of his opinion; but he acknowledges that he had heard ſome account of it; for he ſays, "I underſtood, however, at the time of publiſhing my Obſervations, that he" (Mr. Trye) "confined his views entirely to the diſeaſe in the puerperal ſtate."

That the Doctor has actually *read* Mr. Trye's Eſſay I cannot prove by poſitive teſtimony; but I am able to adduce a chain of circumſtantial evidence, which, I think, will leave no doubt on the minds of your readers, that he has at leaſt *peeped* into the book, and has gleaned from it the principal doctrines contained in his own paper. Mr. Trye's work is in the Library of the Mancheſter Infirmary; and I have good authority for ſaying, it is the uniform practice to circulate the pamphlets on medical ſubjects amongſt the Faculty of that Charity, ſoon after their introduction into the library. Hence it would appear, that this book has been *once* thruſt into Dr. F's hands, and it has been *very often* before his eyes: And when we conſider with what eagerneſs the Doctor has hunted out curious and ludicrous extracts, in order to cram them into his indecent *Illustrations of Sterne*, it ſeems highly improbable, that, when he was engaged in writing his diſingenuous attack on Mr. White's Theory of Phlegmatia Dolens, he would omit conſulting the only other book at that time written in the Engliſh language expreſsly on the ſubject of this diſeaſe. Moreover, the points of coincidence in Mr. Trye's and Dr. Ferriar's Eſſays are ſo numerous, that, in my opinion at leaſt, this circumſtance alone is ſufficient to prove them to be *not accidental.* Mr. Trye has endeavoured to prove, 1ſt. *That Mr. White's theory of the disease is not well founded:* 2dly. *That the proximate cause of the disease is an inflammation of the lymphatic glands or vessels:* 3dly. *That the disease occurs in persons, independently of parturition:* And, 4thly. He has pointed out a *method of cure, founded on his own opinion of the nature of the disease, which consists in the application of leeches and blisters to the*

the limb, &c. Dr. Ferriar has gone over the ſame ground, after the ſhort interval of about ſix years, and yet contends for the originality of his own doctrines! When I gave an Analyſis of Dr. F's Eſſay, it did not occur to me that the Doctor conſidered as an *original ſuggeſtion*, his opinion that the diſeaſe ſometimes happens to perſons not in the puerperal ſtate; otherwiſe I ſhould *then* have proved, that Mr. Trye had anticipated him in this as well as in every other material circumſtance.— Permit me, therefore, to lay before your readers the annexed paſſage, from the 45th page of Mr. Trye's Eſſay: "Laſtly, this kind of ſwelling of the leg, thigh, and buttocks, is not peculiar to women who have ſuffered parturition; I have ſeen it come on, nearly in the manner which Mr. White deſcribes, in a perſon who had ſuffered greatly from a retention of urine and inflammation of the bladder. No circumſcribed tumour could be felt in the iliac regions, therefore it is probable that there was a ſwelling of the internal iliac glands, excited by ſome acrid matter abſorbed from the bladder. The diſeaſe was cured by frictions of mercurial ointment."

I am much deceived, if I have not ſatisfactorily ſhown in another place,* that Dr. Ferriar did not find, as he has aſſerted, the lymphatic veſſels inflamed or enlarged in the limb of Jane Waters. Whether the ſuperficial lymphatics were inflamed, enlarged, and entwiſted like bundles of cord, in the limb of the anonymous gentleman whoſe caſe he has related, may with reaſon be doubted. It is generally underſtood by the profeſſional gentlemen at Mancheſter, that Mr. T. W. of P. is the gentleman alluded to by Dr. Ferriar; and if this opinion be correct, I am informed that the limb has ulcerated ſince the Doctor attended him, and that the Doctor has miſtaken the limb in his Report. How many other errors he may have committed, with reſpect to this caſe, I cannot determine; perhaps he may have miſtaken varicoſe veins for lymphatics!

Having now, I preſume, ſubſtantiated the charge of PLAGIARISM, by *circumſtantial evidence*, as completely againſt Dr. Ferriar as he himſelf has done againſt the celebrated author of Triſtram Shandy, I ſhall next proceed to prove, by *poſitive evidence*, that he has moſt groſsly MISREPORTED the Caſe of Jane Waters.

Dr. F. ſtates in his letter, that his Report on the 21ſt of January runs thus: "The affection of the limb is completely removed. She has no complaint excepting debility, and a cough which ſhe had contracted previouſly to her lying-in." To eſtabliſh the accuracy of this Report, he tells us, that "Home-

* See Eſſay on Phlegmatia Dolens, p. 107.

"Home-patients of the Manchefter Infirmary are thofe who are attended at their own houfes;" that "Out patients are thofe who are able to come for their medicines to the Difpenfary on ftated days." That "on referring to the lift of home-patients, he finds Jane Waters was made an out-patient on the 27th of January, 1798;" and that "the evidence of the home-patient book muft prove, that fhe was able to walk abroad on the 27th of January, 1798." This is the point at iffue betwixt us. I contend that the Doctor's definition of an out-patient is incomplete; that there are out-patients of the Manchefter Infirmary, who are not able to go for their medicines, and that the evidence of the home-patient book does not by any means warrant the Doctor's conclufion. I have already fhewn, (Effay, p. 112,) from the pofitive declaration of Jane Waters, that whilft fhe remained in Manchefter, which was about a month after her delivery, the fwelling of her limb did not abate materially, nor did fhe attempt to clean out her room," and that "fhe was never able to walk, or ftand, whilft fhe ftaid in Manchefter." To her own teftimony, I now beg leave to add that of her hufband, as tranfmitted to me in the following note.

"Mr. Tomlinfon prefents his refpects to Dr. Hull, and informs him that, agreeably to his requeft, he has this day called upon Jane Waters, of Afhley Yard, Fennel Street, Manchefter; that fhe has given him the fame account of the circumftances of her cafe, that is ftated by Dr. Hull; and her hufband has alfo declared to him, *that she never was able to walk during her stay in Manchester, after the swelling of her limb came on*; but that fhe was carried down ftairs on Monday, the 22d of January, 1798, in order to be placed in the coach, which conveyed her to her father's houfe at Newton Heath, and that fhe remained unable to walk for eleven weeks longer, when the abfcefs in her loins broke, and fhe gradually recovered."

King Street, Jan. 8, 1801. (A Copy.)

It may be proper to obferve here, that Newton Heath is at the diftance of three miles from Manchefter; if, therefore, fhe continued to be a patient of the Manchefter Infirmary, fhe muft neceffarily be an *out-patient*, as fhe was removed far beyond the limits within which the phyficians are required to vifit home-patients.

Dr. Ferriar fays, that in oppofition to the *facts* ftated by him, I have produced a "narrative without fome effential dates," and he is "*sorry to say*, without probability." The Doctor's

Doctor's objection to a want of dates is, I trust, satisfactorily obviated by the above note: And I think he has committed an error in observing, that he is *sorry to say my narrative is without probability.*

To invalidate the evidence of Jane Waters, he calls her a poor ignorant creature, and intimates, that her recollection of the circumstances of her complaint must necessarily be *confused* at the distance of nearly two years. To obviate the latter objection it may be sufficient to observe, that I have learnt within these few days, from a professional gentleman of great eminence, and on whose veracity I have a perfect reliance, that she gave *him* an account of her case, very soon after her recovery, which agrees with that published in my Essay on Phlegmatia Dolens, and is, consequently, contradictory of Dr. Ferriar's report in the most essential circumstances. That Jane Waters is ignorant, compared with the Doctor, must be allowed; but she appears to be as intelligent as women in her situation usually are, who in general remember extremely well the circumstances attending their labours and lyings-in, these being amongst the most important occurrences of their lives. And here let me ask, whether learning is requisite to enable a woman to speak accurately to such circumstances as are given in my statement of her case? Will a Court of Law require, that a woman should have read Valentini Novellæ Medico-Legales, Taliacotius de Curtorum Chirurgia, Beroalde, Bruscumbille, &c. &c. before she can be enabled to tell how many months elapsed after delivery, before the swelling of her limbs subsided, and she regained the power of walking? Will the Court of Honour, to which the Doctor has poured out his pitiable complaints, deem it necessary, that Jane Waters should be as deeply read as Dr. Ferriar, in order to tell truly, whether the swelling of her limb was removed in three weeks, or in four months? I presume they will not. Will Dr. Ferriar, though aided by his notes, and the evidence of the Home-patient book, (which he must know does not prove what he has contended for) assert, in opposition to the positive evidence of two credible witnesses, that Jane Waters was able to walk to the Infirmary on the 27th of January, 1798? Will he contend, that she was able to walk abroad on that day? Will he persist in saying that she was ever able to walk across her room, whilst she was under his care?

I shall conclude my remarks upon this part of the Doctor's letter, by stating, that I believe Jane Waters's account of her own case, as published in my Essay, will by the generality of readers be deemed correct, whilst the Doctor's report will be regarded as smelling strongly of the *Farrier's* shop, where the foot

foot is not uncommonly adapted to the ſhoe, inſtead of the ſhoe being adapted to the foot.

One part of Dr. Ferriar's letter ſtill remains to be attended to. He *denounces* my Inquiry into the caſe of Jane Waters as an offence of a moſt ſerious nature, and declares, that I ſhew a total diſregard to profeſſional character by an avowal of it. When the conſequences of this inquiry are conſidered, the Doctor's denunciation is not to be wondered at. He muſt neceſſarily be galled to find it proved to the medical world, that his hiſtory of the caſe is groſſly miſreported, and that his *original* opinions, relative to the cure, theory, &c. of Phlegmatia Dolens, were publiſhed ſix years before, by Mr. Trye.

Towards a man, who had enliſted under the banners of Mr. Simmons, and attacked me in the manner he has done, I do not think it neceſſary to conduct myſelf with more delicacy, or leſs ſeverity. If Dr. Ferriar ſhould henceforward be deemed a Copyiſt, and his Medical Hiſtories be conſidered as wanting the ſtamp of authenticity, he has only himſelf to blame. He ſhould have repreſſed the ill-natured and ill-founded Remarks, which he made upon the Second Part of my Obſervations on Mr. Simmons's Detection, when voted into the library of the Mancheſter Infirmary, if he expected, or wiſhed, the veil not to be withdrawn from his errors.

An, ſi quis atro dente me petiverit,
Inultus ut flebo puer? HOR. Epod.

Mancheſter, Jan. 13, 1801. JOHN HULL.

To the Editors of the Medical and Phyſical Journal.

GENTLEMEN,

I Have not been inattentive to the utility or celebrity of your Medical Journal; and although I have no reliſh for controverſy in general, yet, a Caſe of Monſtroſity recorded in your 19th Number, by Mr. Simmons, appears highly curious, and deſerving of further conſideration. Every one who contemplates the endleſs varieties of animated Nature but with the ſlighteſt attention, muſt admit of the aſtoniſhing wiſdom with which all beings, but particularly thoſe endowed with intelligence, are adapted to the different modes of exiſtence for which Providence has deſigned them. The philoſopher muſt frequently lament, that aided by the profoundeſt accuracy and perſeverance, he can yet aſcend but ſo few ſteps in diſcovering the eſſential operations of mind on matter: Could we once diſpel the

the cloud that obſcures our faculties on this point, phyſiology would acquire ſomething like demonſtrative truth. Although the preſent age is particularly diſtinguiſhed by its thirſt for experiment, and a ſcrupulous adherence to facts on moſt ſubjects of ſcience, yet, a further deſideratum is no leſs neceſſary; namely, caution in generalizing our ideas. We muſt keep in view this axiom, that "poſt hoc, ergó propter hoc," is not always in phyſiology a legitimate concluſion. I ſhall briefly notice a few particulars in the caſe in queſtion, and then take the liberty of ſuggeſting ſuch ideas as my own conſideration of it has furniſhed me with, with due deference, however, to the author of the paper, who is certainly entitled to the thanks of your readers for the communication. I beg leave to premiſe one or two obſervations in this place, and particularly, that caſes of ſuch a ſingular deſcription as the preſent, ſo far from removing the difficulties that occur with reſpect to the illuſtration of the phyſiology of the brain and nerves, rather indicate the ſimple proviſions which Nature can employ in continuing the exiſtence of the human fœtus. The author's mode of reaſoning in the inſtance before us, is not unlike that of a natural philoſopher, who would ground the fundamental ſyſtem of the laws of motion on the eccentricities of a comet. I will juſt quote a paſſage from Steno, which the immortal Winſlow has ſubjoined to his Anatomy of the brain:—"The ſoul which imagines it can penetrate into every thing without it, and that nothing can ſet bounds to its knowledge, is neverthelеſs utterly at a loſs to deſcribe its own habitation, and is no where more to ſeek than at home."—Mr. S. obſerves, that there was nothing preternatural in the ſtructure or relative ſituation of the thoracic and abdominal viſcera, and that they were all ſupplied in the uſual manner with their proper nerves. He ſays, below the occiput there was a conſiderable diſplaced portion of vertebra, both cervical and dorſal, and in theſe parts he found no veſtige of medullary matter; conſequently, I am at a loſs to conceive how he diſcovered the origin of the phrenc. nerve, and the origination of the axillary plexus; for he ſtates, that the upper extremities were furniſhed with their proper nerves. I am induced to hazard a conjecture, that the above caſe was a morbid one in utero, and that it was not originally conſtituted in the ſituation it preſented itſelf in at the birth; particularly, as I am ready to agree with the author in part, that the connection between the brain and circulation of the blood in the fœtal ſtate, is not ſo obviouſly neceſſary. It may not be very remote from the truth, to conſider it as a malformation, produced by partial abſorption; this, however, is merely offered as a query. I am, however, much againſt the author's general

 doctrine,

doctrine, which he has thought ſo clearly deducible from this inſtance: Firſt, that nervous influence is not at all neceſſary in the evolution of the fœtus; indeed, it is anſwered by the univerſal exiſtence of a brain and nerves in the fœtus; and it is well known, that the head is one of the firſt parts of the ſtructure of every animal that manifeſts itſelf after conception; and therefore, we are ſurely not to refuſe our belief in the importance of this ſyſtem, merely from a ſolitary exception. The ſecond poſition, viz. "that the fœtus in utero does not poſſeſs ſenſation," is not leſs liable to animadverſion, ſince, from the different ſtages of its evolution, Nature has appeared to have intended, that the brain in particular ſhould arrive very ſlowly at perfection; between the extremes of ſimple ſenſation and refinement of intellect, the links muſt ſurely be very remote. It is perfectly conſonant to the ſoundeſt wiſdom to believe, that a part which was to be the centre of all our thoughts and affections, ſhould receive acceſſions of phyſical ſtrength in proportion to the neceſſary demands of the animal; if ſo, it is more reaſonable to hazard an opinion, (which, however, can only reſt on that baſis) that the brain and nerves furniſhed an important part in the human frame ab origine; and that ſo far from being a work of ſupererogation at this period, ought we not to conceive it as an additional argument in proof of the unerring rules of Providence, diſplayed alike in every part of the animal creation? Although I have ventured to regard this caſe as denoting partial diſeaſe, ſtill, the healthy appearance of the child otherwiſe, may be thought to weaken this ſuppoſition; nevertheleſs, the author's concluſions are equally liable to our exception. The duplicature of membrane within the cranium and ſpinal canal, would ſeem to favour the opinion of the primary exiſtence of medullary ſubſtance, ſince ſuch a protection to theſe parts would ſeem, a priori, quite ſuperfluous. With regard to the functions of reſpiration and ſenſation being coeval, it does not appear ſtrictly neceſſary, ſince the former, in many inſtances, is perfectly involuntay, and in this reſpect, coincides with the œconomy of the heart. In the caſe before us, one thing is worthy of obſervation, that, notwithſtanding the want of cerebrum and cerebellum, yet we find that the nerves were all within their uſual courſe, (except the optic and recurrents, which were not included in the deſcription) as if they imparted ſome influence, independant of the grand ſource of ſenſation; this idea is curſorily intimated by the ingenious Dr. Baillie in his Morbid Anatomy. Before I conclude, I ſhall juſt obſerve, that I have ſeen an inſtance of an arterial ſyſtem without a heart, in a ſmall fœtus, at about the fifth month; the aorta with its curvature, ſupplied its place. Whatever analogy

logy may be drawn from this fact, I ſhall forbear to point out, ſince truth, not diſputation, is the object of my reſearches.—Thus, gentlemen, I have preſumed to offer a few reflections, which might be uſefully extended on ſo intereſting, but abſtruſe, a branch of phyſiology as the preſent; and if they ſhould accord with the ſpirit of your very valuable Journal, you are at full liberty to inſert them. I am, &c.

Cowbridge, South Wales. JOHN SYER.

CRITICAL RETROSPECT
OF
MEDICAL AND PHYSICAL LITERATURE.

[FOREIGN AND DOMESTIC.]

Geſchichte der Chemie; i. e. Hiſtory of Chemiſtry, from the Reſtoration of Sciences to the end of the Eighteenth Century, by J. F. Gmelin, Profeſſor at Gottingen, Vol. I. 1797, pp. 777, 8vo. Vol. II. pp. 790. Vol. III. pp. 1287.—1798, 1799. Gottingen, for Roſenbuſh.

We ought to apologize for being ſo late in acquainting our readers with a work that may undoubtedly be conſidered as a very acceptable preſent to chemiſts. The author is a man ſufficiently known by the extent of his literary knowledge in chemiſtry, and by the ſuperiority of his learning, which, beſide his particular merits as a chemiſt, render him chiefly calculated for the compoſition of ſuch a valuable work; and in conſidering the labour and indefatigable diligence that is beſtowed upon it, and which few men, even when furniſhed with an equal ſtock of abilities, will hardly undergo, we ſhould ſincerely acknowledge the merits which Mr. Gmelin has acquired by that undertaking, and willingly confer on him that praiſe which he ſo juſtly deſerves. We cannot, therefore, but diſapprove the criticiſm that is written on it by a German Reviewer, who ſeems not to conceive how indulgently ſuch a laborious work ought to be contemplated, and how ſlight defects, and trivial in-accuracies, are abſorbed and loſt in general excellence.

In the preface, the author acquaints his readers, that it is his intention, as a true hiſtorian, to be partial to no ſect, but to follow the paths of truth and impartiality, and neither blindly to adhere to the old doctrines, nor implicitly admire the innovations with contempt of the old.

The

The introduction presents, in agreeable language, a sketch of the progress and changes which chemistry has experienced from its infancy to the present age, and of its state in the different periods and junctures of time. The history of chemistry may be divided *into two chief periods, that* which precedes the institution of learned Societies, comprehending the twelfth and the following centuries, and *that* from the seventeenth century to the present times. In the first time of the former period, chemistry was merely in the hands of the Arabs, which the author, therefore, calls *the age of the Arabs*. The Europeans, however, who followed them, being instructed in their schools, retained, for a long time, their method, doctrines, and prejudices; on which account he stiles it *the age of the Arabists*. This was succeeded by the *scholastic age*, when every science was treated and cultivated after the scholastic manner. The fourth age is that of *Paracelsus*, when that adventurer obtained a superior influence in medicine and chemistry. A great animosity prevailed about that time between the followers of Paracelsus and those of the old doctrines, which parties some enlightened men endeavoured to reconcile; and thus arose, in the first quarter of the seventeenth century, *the age of the Eclecticks*; but as Francis Sylvius de la Boë acquired a particular fame by his doctrines, *the age of Sylvius* was constituted in the second quarter of the seventeenth century. The second chief period is likewise divided into several sections; but the following survey will show the order after which the whole is disposed.

I. Middle History, from the twelfth century to the middle of the seventeenth. 1. *Age of the Arabs*; twelfth century, with a part of the thirteenth. 2. *Age of the Arabists*; the rest of the thirteenth century to the beginning of the fifteenth. 3. *Scholastic age*; fifteenth century to the beginning of the sixteenth. 4. *Age of Paracelsus*; the rest of the sixteenth century. 5. *Age of the Eclecticks*; first quarter of the seventeenth century. 6. *Age of Francis Sylvius de la Boë*; second quarter of the seventeenth century.—All this comprehends the first volume.

II. Modern History, from the middle of the seventeenth century, to the end of the eighteenth. 1. *Age of Robert Boyle*; from the middle of the seventeenth century, to the end of it. 2. *Age of George Ernest Stahl*; from the end of the seventeenth century to the last quarter of the eighteenth; contained in the second volume. 3. *Age of Lavoisier*; from the last quarter of the eighteenth century, to the present times, contained in the third volume; but, by a mistake of the printer, a part of the former age has got into this volume.

The author has given an additional value to his work by the accurate and complete quotations of the different writings of all ages; and, in this respect, it may be considered as the completest repertory for chemical literature.

Triumph

Triumph der Heilkunst; i. e. Triumph of Medicine, or practical Instruction for succouring the most desperate morbific Symptoms, explained by Facts: A Repertory for Physicians and Surgeons. Edited by Dr. Chr. Aug. Struve, Practitioner at Goentitz, Vol. I. Breslau, Hirshberg and Lissa, for Korn the Elder, 1800, in 8vo. (price 1 rixd. 8 gr. or about 4s. 6d.)

The author explains the title of this publication in the preface as "A collection of histories of diseases, which, under the greatest dangers, and most unfavourable symptoms, ended in a most favourable way; instances of the application of heroic remedies and great operations, and of the perseverance, presence of mind, and courage of physicians in the most desperate diseases." The instances and cases are, for this purpose, collected from old and modern medical writings, and the author intends to increase them, in case his plan should meet with approbation. He professes himself, that a great deal of Triumph, in most of those cases, is more owing to Nature than to Art. It were to be wished, the author had followed a better order in executing his plan, which, however, is somewhat retrieved by an alphabetical register, added at the end of the book.

Grundriss der Chemie; i. e. Elements of Chemistry, after the newest Discoveries, and calculated for the Use of Lectures; by Fred. Albr. Charles Gren, Professor at Halle, Vol. I. second edition. Halle. 1800. 8vo. (price 1 rixd. or about 3s. 6d.)

The first edition of this excellent book is known to our readers by a good translation into English. The present one, which has been revised, after the death of the author, by Mr. Karsten, at Berlin, has undergone but few alterations, either relating to some later discoveries, or containing some little corrections. Thus, for instance, the austral earth is omitted amongst the elementary earths; in lieu of which, the glycine, according to Vauquelin's discovery, is adduced. The second part is to follow in a short time.

Traité du Goitre et du Cretinisme, precédé d'un Discours sur l'Influence de l'Air humide sur l'entendement Humain; *i. e.* Treatise on the Wen (Bronchocele) and Cretinism, preceded by a Discourse on the Influence of moist Air on human Understanding. By F. E. Fodere, &c. Paris, for Bertrand, year 8, 1800, pp. 248, in 8vo.

The author, who is already known by his inquiries into the above diseases, gives, instead of a preface, a previous explanation of the word *Cretinism*: "The wen (goitre), he says, is sufficiently known, because it is met with in every country; but not every reader will probably understand what the word *Cretinism* signifies. It is derived from *Cretin*, a name that is given in certain countries to individuals who are perfectly stupid, or fools, and who are likewise called idiots, &c. The word *Cretin* itself comes from Chretien, bon Chretien

Chretien (Chriſtian, good Chriſtian) a title given to thoſe idiots, becauſe they are thought not to be able to commit any ſin. I have adopted this appellation in preference to any other, becauſe the individuals to whom it belongs, are more than idiots, and deſerve a particular name."

In the introductory diſcourſe, the Author examines the influence of moiſt air on the human underſtanding. The Treatiſe itſelf is divided into four ſections. *The first Section* treats of the wen or bronchocele (goitre) of the countries where it is found, of the individuals who are moſt expoſed to it, of the cauſes by which it is produced, and of the different remedies. The Author thinks the remote cauſe to be the humidity of the atmoſphere, and the proximate cauſe a compreſſion or obſtruction of the muciferous canals of the thyroid gland. In *the second Section*, Mr. Fodéré gives the hiſtory of the complete and incomplete Cretiniſm, which is propagated by generation, particularly by the father. The object of *the third Section* is an enquiry into the general cauſe of Cretiniſm and Bronchocele. After having given the topographical and meteorological deſcription of thoſe valleys where theſe diſeaſes particularly occur, and propoſed the different hygrometric experiments made for that purpoſe in different places of the valley of Aoſta, the Author ſtates, that the moiſture of the atmoſphere is likewiſe the cauſe of Cretiniſm; and he endeavours to refute ſome other opinions relating to this diſeaſe, viz. of cold, of diſeaſes of the ſkin, of drunkenneſs being the cauſe of Cretiniſm. In *the fourth Section* the Author ſhows the phyſical and moral remedies that may be employed in preventing the Wen and Cretiniſm. Although theſe diſeaſes ſeem at firſt ſight to be peculiar to the nature of the ſoil, yet by obſerving that the number of Cretins has conſiderably decreaſed in ſeveral places, in conſequence of ſome natural revolutions, and other changes produced by the hands of men, one is entitled to think, that it is poſſible to ſtop the farther progreſs of Cretiniſm by following the rules which experience has furniſhed, and which the Author explains in this ſection; where, at the ſame time, he propoſes a plan of regimen capable of contributing to diminiſh the number of thoſe unhappy creatures. We cannot but ſincerely wiſh, on account of the extreme degradation of human nature in the Cretins, that the philanthropic ideas of the author may be every where realized.

The diminution of the number of *Cretins*, is particularly to be attributed to the draining of the fens, to the more healthy diſtribution, and ſituation of the huts and cottages, the clearing of the woods, &c. by which a conſiderable change in the temperature is produced; and, finally, to the progreſs which of late has been made in education and civilization. The author indicates the proper proceedings for diminiſhing the moiſture of the atmoſphere of the Sub-Alpine valleys, and the means of ſtrengthening the body againſt its noxious influence. The laſt chapter treats of the moral education which is moſt proper to the inhabitants of the Alpine valleys, and on the means of improving the ſituation of the

the unhappy Cretins, and of making the Half-Cretins useful to the community. We conclude the account of this work by remarking, that it is undoubtedly classical on those diseases, and particularly on that deficiency of the human understanding, to which the appellation of Cretinism is assigned, and which seems to be indigenous and endemic in the valleys of the Italian and Swiss Alps.

Histoire Naturelle des Salamandres de France; précedée d'un Tableau Methodique des autres Reptiles indigénes: i. e. Natural History of the Salamanders of France, preceded by a methodical Table of other indigenous Reptiles, with six coloured plates. By P. A. LACRETELLE, Associate of the National Institute, &c. Paris, for Villers, year 8, 1800, in 8vo. pp. 47 and 61.

CIT. LACRETELLE had already presented, in the year 5, 1797, to the Mathematical and Physical Classes of the National Institute, a Memoir on the Salamanders of France, which was very favourably received, and ordered by the National Institute to be printed with the Memoirs of the Foreign Literati. Since that time, however, the author has made new inquiries, of which he has published the results in the work we are here announcing. It is divided into two parts; in *the first* of which, Cit. Lacretelle proposes a methodical arrangement of the other indigenous reptiles, after the classification of Cit. Brongniart. In *the second*, he proceeds to the natural history of the salamanders of France. After having mentioned the modern naturalists who have written upon this subject, and showed how unsettled is the nomenclature of these animals, what they ought to take in the ordines naturales, the author treats of their organization and œconomy, compared with that of other reptiles; of their retirement and re-appearance; and inquires how far we know the generation of the terrestrial salamander; and then proceeds to speak of the act of generation among the aquatic species, on the disposition of their eggs, &c. on their developement, &c. and on their faculty of regenerating the limbs which they accidentally lose, or are taken from them.

Cit. Lacretelle terminates this part with remarks on the sexual difference of salamanders, on their organs of voice, and other peculiarities. He exhibits a methodical table of the salamanders of France, in French and Latin, and concludes the whole with a description of the species. These are, 1. *La salamandre terrestre*, which is figured in the first plate; and of which the second exhibits an accurate figure of the skeleton. 2. *La salamandre marbrée.* 3. *La salamandre cretée*, i. e. the crested salamander. These two species are represented on the third plate; and the fourth exhibits the female of the crested salamander with opened belly, to show the situation of the intestines. 4. *La salamandre abdominale*, engraved in plate 5, fig. 4, on which likewise two young ones, in their different state, are represented. 5. *La salamandre ceinturée*, with a girdle, figured plate 5, fig. 5. 6. *La salamandre ponctuée*, or the dotted salamander, of which male and female are figured plate 6. fig. 6. A. and fig. 6. B. 7. *La salamandre palmipéde*, engraved

graved plate 6, fig. 7. and a variety of it with its feet more palmipede, fig. 7, B.

Prof. Reich's Theory and Practice of Fever.

[Continued from Vol. IV. pp. 556—563.]

"Since that time," Mr. Reich proceeds, "I became more acquainted with the great uſe of mineral acids, and employed them in more or leſs quantity in all caſes of fever, whatever appellation they may have, acute, remittent, ſimple, complicated, idiopathic or ſympathic, ſymptomatic or original, acceſſory, inflammatory, putrid, pituitous, bilious, exanthematous, nervous, lanteſcent, epidemic, contagious, &c. I was convinced that they alone are capable of curing every fever, in the ſhorteſt ſpace of time which can naturally be imagined, and of removing all danger, when no organic leſion exiſts in any part of the body, and no error is committed either by the phyſician, or the patient and thoſe who attend him.

"Having for a long time only uſed the ſulphuric acid, and often experienced that the patients did not reliſh it, nor that it acted ſpeedily enough, on account of its not poſſeſſing that degree of volatility to emit the oxygen quick enough, and of its cauſing ſome inconveniencies to the ſtomach, I made the experiment of giving the muriatic acid internally, in the ſame quantity, and even in larger doſes, being more volatile than other acids; and as it proved, in all caſes, as uſeful as could be wiſhed, I preferred it to ſulphuric acid, both of which are the principal arcana the diſcovery of which I thought ſo intereſting to humanity. It is however to be wondered, how particularly the muriatic acid could have been ſo long neglected, as the extent, and the abſolute neceſſity of its uſe, in its combination with natron as common ſalt, ſhould have long ago engaged the attention of medical men; and it is from the above experiments, and theſe theoretical arguments, that I am induced to allow, with reſpect to medicinal quality, the preference to the muriatic acid; and though it has not yet been decompoſed by a chemical proceſs, it is, however, a matter of the higheſt probability, that this is effected in the organic body, as neither the ſalt itſelf, nor the muriatic acid, is excerned from it in ſubſtance, after being internally taken.

"Beſide the ſulphuric and muriatic acids, which, with reſpect to their intenſive ſtrength, ſurpaſs the reſt, I have alſo made uſe of other acids for the ſake of putting my principles to a fair trial, and it is with ſatisfaction that I found them in this way confirmed. The nitric acid proved frequently very uſeful to me in dyſenteries and chronical painful diarrhœas; I do however but ſeldom employ it now, becauſe, beſides its being much inferior to muriatic acid in point of volatility, it is imperfectly decompoſed in the body, and only a part of oxygen diſengaged from it, whereas the greateſt portion remains in the body, combined with oxygen in form of nitrous acid, which merely differs in degree from that matter, which,

which, according to the ingenious theory of Prof. Mitchill, ought to be esteemed the frequent source of the most dangerous epidemick diseases. I observed likewise, that considerable swellings often ensued the application of it. The phosphoric acid, which I have also sometimes used, did never effectually operate in urgent cases; and as it is the most fixed of all, little effect can in general be expected from it.

"The super-oxygenated muriatic acid, however, is one of the most efficacious, agreeable, and cheapest remedies, from the internal use of which, I have frequent instances of the best success, particularly when oxygen was required to be brought quickly to the organs, as in cases of lethargy, &c. though it is by far inferior to simple muriatic acid, which contains a greater intensive proportion of oxygen than this. The vegetable acids have but little effect, because they contain a great quantity of hydrogen and carbon in their mixture, but they may however prove of some avail in slight febrile affections.

"The best way of administering all these acids, is to bring them immediately into the organs of digestion; and the chief place of their action is the stomach. They may, however, be applied with success in clysters, and likewise externally on the skin, in baths or fomentations, in both which cases they ought also to be sufficiently diluted.

"But the following objection may be raised against the use of acids, according to the above theory, viz. that fevers are removed without any application of acids, by quite different remedies, and that consequently their use is not so absolutely necessary as is here asserted, and that the whole construction of the above principles is therefore destroyed. To this I answer in reply, that the known febrifuge remedies are either substances compound of mineral bodies and acids, or vegetable substances, that Nature has provided with more or less oxygen, in a free or bound state, which are decomposed into their elementary particles by the chemical and galvanic laws of the body. It may now be easily conceived, how mineral bodies act against fevers, and likewise vegetables that are endowed with free oxygen or acid; but how vegetable substances not containing any acid, are capable of repressing a fever, is still a matter of inquiry and discussion. It ought however to be advanced here, that acid and oxygen are two things utterly different from each other, the first being a compound, combustible substance; the latter simple, uncombustible, and not to be represented by itself, whose existence can only be conjectured, but not perceived by our senses. As the life of the human organic body continues only by the mutual action of opposite principles, of which oxygen acts as the negative power, it is also probable, that this principle is according to the inconteſtible laws of Nature, necessarily requisite for vegetable organism, and consequently contained in every vegetable substance in greater or less proportion. Thus, for instance, the *bark* is, according to the researches of Mr. Fourcroy, provided with a very considerable portion of oxy-

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gen, which alſo, after my experiments, is found in the different indigenous barks and woods that have been propoſed as ſubſtitutes for it. Aromatic plants, reſins, eſſential oils, ſpirits, ethers, and the camphor, contain a great deal of oxygen; and opium even poſſeſſes much of it. The ſtate and degree of oxydation in vegetables remain ſtill to be aſcertained by further reſearches, but whether this may be performed through the medium of a retort, is very much to be queſtioned. It is probable, that if thoſe remedies do not operate in the body, by immediately imparting oxygen to it, they either combine themſelves there according to the laws of affinity with thoſe ſubſtances, by which the oxygen was bound in it, which is in this way diſengaged and left to operate, or they produce an alteration in the ſyſtem, by which the ſame thing is effected. Thus may the action of the excitant remedies be explained, which are frequently of ſuch great avail in the laſt periods of fevers. But as it is in the power of Art to reſtore the want of oxygen by remedies that immediately operate, it ſeems more proper rather to employ them, than to have recourſe to thoſe that only have a mediate effect.

"It may in ſome caſes however be adviſable, for the ſake of raiſing and multiplying the affinity, to give alternately, or at the ſame time, the mediately operating remedies with thoſe that act immediately; but theſe caſes ſhall be in future more ſpecially determined. That emetics, purgatives, and clyſters, have ſometimes a very good effect in fevers, by evacuating at once the matters, from which a new ſtimulus febrilis is always evolving itſelf, can likewiſe not be denied. Baths alſo, and fomentations, contribute a great deal to the cure of fevers, by reducing or diminiſhing the warmth according to their temperature with reſpect to the body; but it has been above mentioned, how much their effect is heightened by the addition of acids.

"The quantity of acids internally requiſite for the cure of a fever, cannot be well aſcertained, and merely depends on the ſucceſs; but intelligent phyſicians will be able to adminiſter them in ſuch a manner, that the expected effect, the cure of fever, is obtained without injuring any organ neceſſary for life.

"It has been laid down as a general rule in curing diſeaſes, always to adjuſt the doſe of any remedy according to the different circumſtances; that is to ſay, according to the previous degree of fermentation. The above mineral acids ought therefore to be adminiſtered, in beginning and increaſing fevers, in a moderate doſe, often repeated, viz. from one drachm to half an ounce in eight ounces of water, and ſome ounces of any ſyrup; or if indicated, mixed with any ſpirituous ſtimulating ſubſtance; to be taken every hour or every two hours, from one to two table-ſpoons full, or half a coffee cup, to which doſe water muſt be mixed, or drank ſoon after. In time of danger, however, and in the period of criſis, one to two drachms, (40, 50, 60, to 100 drops) may be given at once, and repeated if thought proper. The ſulphuric acid being ſtronger than the muriatic and nitric acid, leſs is required

quired of it than of these; the super-oxygenated muriatic acid however, may be administered in large quantities, from one to two ounces every half hour or every hour. I have myself taken, in the course of four hours, a whole quart of it, and several of my patients took in the same time twelve or more ounces, without experiencing any other symptom than three or four watery stools.

"Although the intensive strength of any of those acids is not to be much attended to, as every thing depends on the event alone, and if 40 or 50 drops do not produce the expected effect, the dose ought to be increased or oftener repeated, yet it facilitates the whole proceeding, when acids of equal strength are always employed. How long the acids must be continued is only determined by different circumstances; and while in some cases 100 drops are sufficient, others may perhaps require 800 drops. And the more the fever approaches to the highest degree of fermentation, the less is wanted for its termination and cure

"Several inconveniencies accompany the use of mineral acids, which frequently oppose unsurmountable difficulties to the physician. The very disagreeable taste of acids can only be somewhat remedied by sufficiently diluting and sweetening them with juices that are pleasant to the taste. The soreness of the lips and of the internal surface of the mouth, sometimes observed, may generally be thought to be occasioned by not having taken sufficient care to involve and dilute them; but most frequently it originates in the vehemence and malignity of the fever itself, that brings on a particular disposition to such excoriations. There is nothing to fear for the stomach on that account, when they are given with proper care; and they are indeed not more detrimental to the stomach than pepper, horse-radish, mustard, &c. which substances, though causing blisters on the external surface of the body, are taken without any harm internally. The disagreeable sensation of the teeth, which is attended with the frequent use of acids, is of no great consequence, and disappears on the discontinuance of them.

The symptoms of a favourable event from the use of acids in fevers, differ according to the different kinds of those affections. Eructations, and even vomition, at the time of deglutition, gurgling of the bowels, discharge of wind, diarrhœas, rising and sinking of the pulse, change of heat and frost, sweats, salivation, increased secretion of urine, rest and sleep, &c. are generally the symptoms from which, in urgent cases, good success may be hoped. The most favourable symptom however is, when the senseless patient awakes from his stupor, and recovers his senses, or falls into a sound sleep, in which the pulse becomes slower, more regular, and moderately full.

"Deadly symptoms generally were, according to my observations, when purple spots appeared in the face, when one eye was half opened while the other was shut in a paralytic manner; when the cornea became again opaque, after it had been previously rendered clear by the use of those remedies; when the patient falls back into senselessness, attended with a facies hippocratica; when a rattling

a rattling in the throat is perceived, and the pulſe becomes intermittent and irregular.

"After having removed all danger, and baniſhed the fever by the acids, it is not abſolutely neceſſary to employ other medicines, and I have, except in few caſes, given no other remedies, but left the entire recovery to Nature, aſſiſted by a good and nouriſhing diet."

Theſe are the chief principles of Dr. Reich's peculiar ſyſtem of fevers, which, as he pretends, will enable every practitioner to cure every kind of fever, under whatever name they may be recorded in the annals of medicine. He promiſes to give a more copious and full explanation of them in a particular work, which will ſhortly appear. However conſcious of the imperfections that may be ſtill imputed to his ſyſtem, he does not dread the trial of his principles, as he eſteems them to be particularly calculated for uniting all parties and all ſymptoms. Nobody, he thinks, whoſe conceptions of a chemical proceſs are not limited by groſs and vulgar ideas, will ever imagine, that by generally adopting the principles laid down here, the times of Sylvius are likely to return; but fully perſuaded of the neceſſity of raiſing the art of healing to that point of view, he ſhould feel the utmoſt ſatisfaction, if the path were purſued he had attempted to tread.

Profeſſor Reich proceeds now to point out the advantages which he pretends to reſult from his diſcovery for medicine, as well as for the community; and we ſhall relate them, but without entering into their ſpecial explanation, as he moſtly appeals to his previous theoretical arguments. Theſe advantages are the following:—

"1. The medical ſcience will thereby obtain a ſurer and firmer baſis, upon which a more accompliſhed ſyſtem may in future be conſtructed, by farther obſervation than we have hitherto had.

"2. All febrile diſeaſes, without exception, will be contemplated from a better point of view, and, of courſe, more ſpeedily and ſucceſsfully removed; and every practitioner will be enabled, upon more rational grounds and principles, to diſpel all danger during a few hours, in caſes where no particular complication exiſts, nor the organs, neceſſary for life, are too much hurt: Farther, by that method of cure, a ſhorter duration of thoſe diſeaſes will be occaſioned, the moſt generally bad and detrimental ſymptoms prevented, and ſtrength of the body in the eaſieſt and quickeſt way reſtored.

"3. Certain diſeaſes, which have hitherto been thought to be incurable, or very dangerous, and only in ſome caſes cured by empiric remedies, will, according to thoſe principles, be better underſtood and more ſucceſsfully treated." The author means by them the hydrophobia, the plague, yellow fever, and pulmonary conſumptions; and though he has not had an opportunity of treating the three firſt diſeaſes, he is convinced, by rational arguments and his ſucceſs in other fevers, that acids, particularly when given in the firſt periods of thoſe diſeaſes, will have the moſt ſucceſsful effects. With reſpect to conſumptions, he aſſerts that he has ſaved the

the life of many conſumptive people, or, at leaſt, conſiderably relieved them; and on that account, he recommends to his patients an acid diet, and orders them to drink fluids mixed with muriatic or ſulphuric acid, and ſpiritus vini; in which way their ſtrength and health were entirely reſtored, as far as the ſtate of the lungs would admit of.

" 4. Epidemic, putrid, or nervous fevers, diſeaſes of camps and hoſpitals, by which thouſands are deſtroyed, will in future be cured with ſecurity and leſs difficulty, and at the ſame time in the cheapeſt and moſt ſimple way.

" 5. By the diſcovery of thoſe remedies, and that new method of cure, field phyſicians will be enabled to prevent the riſe and progreſs of ſuch diſeaſes, as far as this lies in the power of man." He recommends, therefore, to allow each ſoldier, on any long and weariſome march, a certain portion of elixir acidum Halleri.

" 6. The mortality of the moſt deſtructive epidemic diſeaſes of children, as meaſles, ſcarlet fevers, ſmall-pox, hooping cough, &c. will be more ſecurely and eaſily prevented than has hitherto been the caſe;" and, conſequently,

" 7. The great mortality of children will be greatly diminiſhed by adopting thoſe principles: The mortality of children in the infantile age, originates in the falſe ideas on the cauſe of the diſeaſes to which they are expoſed, as they are generally aſcribed to an acrimony in the ſtomach, againſt which, alkaline and abſorbent remedies were employed; but the little good they did, conſiſted in the carbonic acid that was, through their medium, brought into the body. The mineral acids, however, prove, according to Mr. Reich, of the greateſt avail in all the different affections of the infantile age, difficult dentition, diarrhœas, convulſions, &c. The general preſcription is, to mix with two ounces of any ſyrup and the ſame quantity of water, half a drachm to two drachms of ſpiritus vitrioli, or muriatic acid; half a drachm of liquor anodynus, or ſpiritus vini rectificatus, of which mixture, every hour, or every two hours, one or two table ſpoonfuls are to be taken; when the diſeaſes are attended with much pain, ſome laudanum, or tincture of opium may be added.

" 8. The treatment of ſeveral non-febrile diſeaſes will, by that method, be conſiderably and ſucceſsfully reformed.

" 9. The remedies by which the cure is performed, are very cheap, and, at the ſame time, fevers are cured in a ſhorter period with leſs expence, whereby conſiderable ſums will be ſaved, that are exported for foreign drugs."

Such is the Theory of Dr. Reich, and the method of cure which is grounded upon it. Although we may not deny that it agrees with the new diſcoveries in chemiſtry and natural philoſophy, yet, it muſt ſtand the teſt of experience and obſervation before it is likely to meet with general approbation. Of twenty-eight patients who were treated by Dr. Reich in the Charité Hoſpital, under the ſuperintendance of the Committee, three died, but the reſt were cured. In private practice the remedies were uſed with different

ferent succeſs, and ſome obſtinate quartans and tertians were removed by them; while, in other caſes, they proved of little or no avail.

We ſhall not omit communicating to our readers the further tranſactions relating to this theory, which are likely to be publiſhed by different phyſicians.

An Essay on the Malignant Pestilential Fever, introduced into the West Indian Islands from Boullam, on the Coast of Guinea, as it appeared in 1793, 1794, 1795, *and* 1796. *Interspersed with Observations and Facts, tending to prove that the Epidemic existing at Philadelphia, New York, &c. was the same Fever introduced by Infection imported from the West India Islands: And illustrated by Evidences founded on the State of those Islands, and the Information of the most eminent Practitioners residing on them.* By C. CHISHOLM, M.D. and Inſpector General of the Ordnance Medical Department in the Weſt Indies. Two vols. 8vo. about 1000 pages. London, Mawman. Price 16s. boards.

THE Author, in his Preface to this ſecond Edition, ſays, "I have exhibited the motives which induced me to preſent the public with a ſtatement of my opinion reſpecting the nature, and of my reaſons for adopting a new mode of treatment of the Malignant Peſtilential Fever. Any further addreſs might ſeem unneceſſary, and would, in truth, become ſo, had not much, and in ſome inſtances illiberal, controverſy taken place, tending to ſubvert that opinion, and to diſcredit that treatment. How far the authority, on which reſts the vehement oppoſition of theſe controvertiſts, may be relied on, is certainly an object of importance: But as no facts ſupport them—as no local knowledge or information appear to have been acquired—and as their aſſertions ſeem to be founded on the baſeleſs fabric of theory, and the generally received but falſe idea of the incompatibility of a heated and rarified atmoſphere to retain infection—it muſt neceſſarily remain with me as viſionary, or, at beſt, equivocal. And I am the more inclined to conſider this oppoſition in a light which reflects little credit or reſpect on the authors of it, becauſe the reſult of my own obſervation, and of the enquiries which I have induſtriouſly made, during my later reſidence in the Weſt Indies, has only ſerved to confirm me more ſtrongly in the truth of what I have already offered on the ſubject. But, as the public poſſeſs a right to more than ſimple aſſertion on my part, I have judged it proper to intrude myſelf once more on their attention, and to offer my ſentiments in a form more diffuſed, and more capable of fixing the mind, than that of the firſt Edition of my Eſſay on the Malignant Peſtilential Fever.

"In the execution of this deſign, I have thrown the following work into four parts. The firſt contains an account of the origin, progreſs, diagnoſtic, cauſe, and other circumſtances of the pathology of the diſeaſe; each of which is treated of in a diſtinct chapter. The ſecond relates the means of cure, in five chapters; the three firſt are appropriated to the diſcuſſion of the three general indications

dications of cure, the result of the consideration of the circumstances peculiar to the disease. The fourth is employed in the discussion of the principles on which the efficacy of mercury in fevers of hot climates in general, and more especially in the Malignant Pestilential and Yellow Remittent Fevers, is founded. And the fifth is confined to such observations on the action of oxygenated medicines on the system, deranged by fever and other morbid affections, in hot climates, as have occurred in a very extensive use of them. For this, which, on a general view, seems to have little connexion with the subject of the Essay, little apology perhaps is necessary; and the late discoveries relative to the share which the gaseous fluids have in almost all curative intentions, may preclude the necessity for any. The third part is altogether allotted to the important investigation of the means of prevention. In the course of this enquiry a question of infinite consequence is started, and an attempt made to display the means by which the purpose of it may be fulfilled; I mean how far the European constitution may be prepared for the process of assimilation to the tropical climates?

"Having thus considered the great outlines of those divisions of the following work which relate to the pathology, the cure, and the prevention of the Malignant Pestilential Fever, I shall proceed to account for the publication of the fourth part.

"The opposition which I have had, and have to contend with, in my endeavour to impress the truth of my sentiments on the public mind, relative to the origin and cause of the propagation of the pestilential infection, which has characterised the late direful epidemic; and to the mode of treatment which I, as well as every unprejudiced practitioner in the West India Islands, have found the only successful one, has proceeded from the agents of the Bulama Association in the first instance, and from the Medical Staff of the Armies acting in the West Indies subsequent to the year 1795. The attempts at refutation of the strong facts which have occurred at Grenada, by the agents of the Bulama Association, are similar to the assertions made by the writers for the Levant Company, against the importation of infection into Great Britain immediately from Turkey. The first have imagined that their colonies on the coast of Africa might be injured; and the latter had fears of their trade suffering: we see therefore in both, interest overleaping every obstacle, and shutting the door against truth. The conduct of the gentlemen composing the medical staff cannot be so obviously accounted for Was it necessary to exhibit causes for the wide devastation which took place in the military hospitals, we might expect a delineation of facts tending to prove that this proceeded from the faithful observation of the method recommended by me; but what shall we say, when we are told that the efficacy, and even danger, of this method, were inculcated at the commencement of the operations of the armies, and before cases occurred on which trials might be made. It is not my intention to enter on the invidious task of displaying the real causes of the loss of 13,437 soldiers, our countrymen, in a period little exceeding thirty months, by the Malignant

lignant Pestilential and Yellow Remittent Fever; but I may, without incurring censure, ask, why the mercurial treatment was not recurred to, when that adopted had so generally failed? No satisfaction will arise from being told, that much of the dreadful mortality which took place proceeded from the latter of these diseases; for the applicability of the question to the nature of the event is not diminished. I should not, however, consider it as necessary to state the general circumstances of the medical conduct of the Hospitals, did not the accumulations of death from 1796 to 1798, present an argument to the uninformed, against the utility and propriety of the mercurial treatment of the fatal maladies in question. To such the evidences which the fourth part comprises may be useful; for my own justification I consider them as necessary. A vain and absurd attempt was indeed made in the year 1797, to shew that the exhibition of mercury, in the fatal epidemics which prevailed during that and the preceding year, had been generally recurred to; and that the general result was far from favourable; but the reports on which this belief was founded, were of such a nature as discredited the position which they were intended to establish; for it is universally known, that the mercurial treatment was industriously condemned by those placed in the direction of the hospitals; and a youthful, inexperienced, and docile staff readily trusted, where attention, application, and a denial of accustomed indulgence, would have been the price of success. Were there a possibility of inaccuracy in this statement, I would rejoice in suppressing it. I am perfectly open to conviction, and feel no enmity to any individual of the staff; but justice to myself, and humanity, shuddering at the destruction of nearly the whole of the West India army, call upon me to declare that there is undoubted authority for stating, that notwithstanding the total inefficacy of the remedies generally employed in the general hospitals, the practice described by me was not recurred to by more than three army physicians in a fair and judicious manner. Two of these gentlemen are unhappily no more; but Dr. Wright is a living monument of the advantages which result from the employment of mercury, conducted by professional skill, and by judgment. This gentleman's report has been made public, and the world unites in the eulogium of the author.

" The absurd conduct of the Medical Board at home, at the time Sir Ralph Abercromby's army was formed, is chiefly reprehensible; and may indeed, very justly, be considered as an extenuation of the inexperience, the folly, or the crime, of the younger members of the medical staff of that army. In the name of common sense, let me ask, what useful purposes could arise from a regulation which precluded all from exercising the important functions of an army physician, but such as derived their medical knowledge from seminaries where none can be obtained; or such as were, or became licentiates of the London College of Physicians? What had an Oxford or a Cambridge degree; what had a licence to practise within seven miles of London, to do with medi-

cal practice within the tropics? Can the whole of that learned body united, communicate that experience in the treatment of inter-tropical fevers, which can be acquired only by a long and painful attention to them in the torrid regions where they are endemic? This singular infatuation, by which the order of things was inverted, proceeded from views best known to the triumvirate themselves; but its effects may be considered as the remote cause of all the misfortunes which befel the devoted troops composing the army under the command of Sir Ralph Abercromby. Our country, therefore, have inexpugnable reasons for consigning a Board, whose resolves and regulations militate so forcibly against the sober dictates of common sense, and whose self-sufficiency has given existence to a cloud which obscures the most manifest proofs of the baleful tendency of their measures, to the fate of the unhappy victims who have fallen under the operation of both.

" The fourth part of the following work is intended to constitute, therefore, a body of evidence, to shew the fallacy of the statements and reasoning of the Agents of the Bulama Association; and to exhibit the disingenuous conduct of the medical staff, and the fatal consequences of a disbelief of the efficacy of a remedy which had been established by the experience of a multitude of eminent practitioners in all the West India Islands, where the calamity it was calculated to correct, existed. In treating this part of my subject, the views of the actual state of each colony, I have given, may be considered as too diffuse, and unnecessarily minute; but I have found the matter which presented itself, important; and the shades of variation are too considerable to remain unexamined in the investigation of morbid causes. Those objects which are common to all, have been sufficiently explored in the Introduction to the first Edition, which I have therefore given a place in this.

" Dr. Trotter, of the Navy, has thrown out Remarks, the tendency of which is, to invalidate the authority on which I found my opinion relative to the origin, the nature, and the treatment of the Malignant Pestilential Fever. It would be with no uncommon concern I should enter the lists of controversy with a gentleman whose liberality of thinking, professional skill, and exertions in the cause of suffering humanity, have placed high in the estimation of the public; and I trust that a more attentive consideration of the facts and reasoning I have submitted to the public in the following work, will remove that inclination to differ from me, which is only too apparent in several passages of his excellent work, The Medicina Nautica."

Further Information respecting the Case of Mrs. Craib, formerly announced as cured of a Cancer in both Breasts by Dr. Nisbet. With additional Cases: Communicated by Mr. OLIPHANT.

To the EDITORS of the MEDICAL and PHYSICAL JOURNAL.

Gentlemen,

IN the laſt Number of your Journal, there appears a "Reply to Mr. Oliphant's Account of Mrs. Craib's Caſe of Cancer," wherein it is ſtated, that my Communication was "clearly meant to imply a reflection on the former relation;" whereas my motive, I ſolemnly declare, was what I thought due to myſelf and the public on this occaſion, viz. ſolely to give a faithful detail of the caſe of Mrs. Craib; which I did, and ſincerely conſole myſelf in ſo doing, for reaſons hereafter mentioned.

I cannot help obſerving, in order to leſſen the dependance on my authenticity, and to draw aſide the public's notice from my report of the caſe, ſo eſſentially different from his, that much ingenuity has been employed by Dr. Niſbet to ſerve his purpoſe; this, with thoſe who know me, will weigh nothing; and will weigh juſt as much with others when they are acquainted with the ſequel.

The gentlemen advertiſed in his firſt communication had very little to do with the detail of Mrs. Craib's caſe; therefore I conſidered particular notice was irrelevant in my report; only two preſcribed, the reſt merely looked at it; they did not all give an opinion: yet an effort has been made to degrade their profeſſional abilities before the public, under his ſpecious pretence, merely to give them "an opportunity of proving the authenticity of the ſtatement;" which, I believe, they did not give themſelves the trouble to do; whereas, in fact, it was to enrich his own triumph and influence in the public opinion. When there were collected together, by private invitation, ſeveral other gentlemen of the profeſſion, to dandle this abortive unique, ſurely the like gentle mode might have been extended to the other reſpectable ſurgeons, if a ſimple opportunity had been alone in contemplation: No, it could not here be ſo intended, though

"Inter ſe convenit urſis."

My intention being a detail of facts, not controverſy about imaginary points, however wiſhed for, in the relative deſcription of diſeaſed appearances, the moſt eſſential difference will be

be ſeen: No reverted edges, no pouring out of quantities of blood; no rheumatic or erratic pains, except a few hours of pain from very hard labour; no violent pain in the breaſt, or large ulcer, ever happened: there were ſome alarming chronic inflammations in the miliary glands of the ſkin, and a red ſub-livid hue of the whole breaſt; the laſt was the conſequence of accident and empirical treatment.

I did not object to the frictions, in the firſt inſtance: Being reſponſible for a ſafe conduct of treatment, when my patient was liſtleſs and greatly reduced, with loſs of appetite, and a ſore mouth, in extremely hot weather, not favourable to the regaining of loſt ſtrength, by her ſolicitation, and my own opinion, I deſired that the ſevere diſcipline might be laid aſide. There was no ſhrinking of the breaſt perceptible, but what was in common to the reſt of the body, except as far as aroſe from an extenſion of the ulcerations, till after the pleuritic attack.

The reſolution of the upper and larger part of the diſeaſed breaſt into matter, was often ſaid to be at hand, and in an actual ſtate of ſuppuration, though it never happened; though felt, it was never ſeen.

Of the primary cauſe of abſorption of the breaſt, and the altered condition of the ulcerations, I have given an opinion in my ſtatement; wherein I neither took the merit of a cure to myſelf, nor allowed it to the Doctor: though I admitted the breaſt was abſorbed, and the ulcers healed, yet a conſtitutional diſeaſed irritation ſubſiſted from this abſorption.

If it is meant, by the "impropriety of the application," (pronounced, I am ſure, by no oracle in phyſic,) that my treatment of the patient in the latter week of October was improper, when I conſidered it uſeleſs to conſult him, I muſt ſay, (by the violent interpoſition of Dr. Niſbet, who, unſolicited, called in another phyſician to give weight, and carry the patient by a coup de main,) a decoction of Anguſtura bark, with vitriolic acid, was preſcribed by him; and though the practice was irregular, I made up this medicine, having alone a wiſh to benefit the patient, and to convince her that ſhe ſhould have all the aſſiſtance ſuggeſted for relief: I gave her, however, my opinion, on adminiſtering the firſt doſe. By this treatment a ſtop was put to the plentiful glary expectoration; a moſt teaſing cough came on; tightneſs and oppreſſion acroſs the cheſt, with ſhort breath; ſo that I had the greateſt difficulty to perſuade her to repeat the doſe, and only on the aſſurance that ſhe ſhould have a large delaxant portion of ipecacuanha, with opium, and ſome volatile alkali, to provide againſt ſerious conſequences, ſhould they enſue; a ſecond portion was given, but might have proved fatal, (according to the account of her friends

friends who attended, and Mrs. Craib herſelf, the following morning,) if ſhe had not taken the provided medicine. Though the Doctor ſuppoſed he had cured the cancer, he puſhed further here in the province of medicine.

Did I treat my caſes "with bark and carbonic acid?" Has Dr. Niſbet compiled ſo much, and yet in a trifling ſtatement is capable of making ſuch a miſtake? This error is of itſelf ſuſpicious. However, I ſhould not be ſurpriſed to find the greateſt enemy to bark in detail, employ it in practice, and without reſtriction: ſuch to me ſeems to be the caſe with opium.

I muſt take notice alſo, that the reflector on Mrs. Oſborn's caſe has paid leſs attention to the circumſtances of it, than to his own reveries: the ſkin was puckered before inflammation, ſuppuration, or ſloughing; but after ſloughing, the reſerved ſkin extended ſmoothly over the formerly ulcerated ſurface, not healing as a ſcrofulous ſore, the appearance of which every tyro, nay, every old woman is acquainted with.

As to his making a compariſon between the influence of ſudden inanition and ſtarvation, in removing morbid accumulation by abſorption, it is only an interlude.

It is true, Dr. Niſbet aſked me, if I had any objection to his giving Mrs. Craib's caſe to ſome periodical publication; and I ſaid, none. When he had written it, he waited on me in his way to the receiver of communications, and read it over. To the beſt of my recollection nothing particularly paſſed, except at our parting from my houſe, wherein I told him, his account, with regard to my opinion given to Mrs. Craib, when I firſt ſaw her, was not true; and a proof of this he could have from herſelf. My opinion was, that I gave her hopes of a ſloughing of the whole diſeaſed breaſt, not "by abſorption," as was afterwards inſerted; and alſo an improbability of any of it being reſtored to health. Before he read me his account, as in the other reports of opinions, it was repreſented, in the moſt unqualified pronunciation, to be incurable, and that he of himſelf had performed a perfect cure. Whether he would have made any alteration from what I ſaid, I knew not; indeed, it was of no conſequence.

Mr. Coe, Mrs. Craib's friend, and Dr. Nisbet's operator, a worthy man, though in a humble ſtation of life, called upon me the ſame evening; and it was mentioned to him what his acquaintance Dr. Niſbet intended; the manner and precipitancy of which I diſapproved of. He ſaid he would go to the Doctor, as it ſtruck him in the ſame way; and would talk to him about it. He called again, and told me the Doctor replied, "he would ſtop its publication."

What

What right has any one to judge of another professional man's sentiments, who had not himself pursued "the candid line of conduct," and had broken off all communion by his empirical conduct, and reduced himself below the level of a regular practitioner?

With regard to joint names, was it likely I would make myself responsible for any thing Dr. Nisbet might have chosen to insert? for, in fact, I had not even the reading of it; yet I was to bear its fate for the Doctor's sake. This is a fac simile to his declaring, in a note to Dr. Bradley, that leave was given, by gentlemen who had seen the case, to attest the cure by the use of their names: whereas, in truth, Mr. BLAIR, one of them, did afterwards publish a gentle rebuke and refutation of such a tender on his part.*

As far as hitherto concerns our statement of Mrs. Craib's case to the public, the patient herself had read both; and was asked, in Mr. BLAIR's presence, her opinion with regard to their justness: Her answer was, "If she had not been told Dr. Nisbet's statement was that of her disorder, she could not have thought it; but that, as far as she could judge, or knew, Mr. Oliphant's account was just."

My statement in the London Medical Review and Magazine for December, so far from advancing that I had cured the patient, according to Dr. Nisbet's attack, will be found to contain these words: "However, subsisting still is a *scirrhus* in the right breast, which seems to enlarge with her repletion; and there is not the least doubt of morbid irritation existing in her habit, most likely the offspring of her absorbed diseased breasts, or that disposition which produces scirrhus."

I am sorry my opinion has been realized. On the 1st of this month she sent for me, to shew me a few eruptions that had come out the preceding fortnight; and with considerable relief to her inside, she told me. When I saw her, I found there were innumerable affections of the miliary glands of the skin of both breasts, particularly under the absorbed one, extending a great way on the side under the arm. The last healed sore, where the absorbed one was, was re-opened; on several other parts of this surface considerable scirrhous tubercles have arisen. The right breast, from being left with a small pendulous scirrhus, sustained by the flaccid integuments; the skin has now contracted, which draws up the increased scirrhus; and the whole threatens to become one mass of disease, of much more serious consideration to Mrs. Craib than any

* Medical and Physical Journal, No. XXII. p. 569.

any former ſtate of her malady that I have witneſſed: as alſo her preſent weakneſs, indifferent appetite, bad digeſtion, a diſpoſition to anaſarca, and her pulſe upwards of 112, have diſabled the conſtitution from ſuſtaining ſuch a grievous load for any great length of time.

Leſt this farther account of my patient's caſe ſhould be called in queſtion, from ſiniſter motives, as had already happened, I requeſted Mr. Blair to ſee the ſtate of Mrs. Craib's health on the 8th inſtant; which he politely and obligingly did, and favoured me with his report of what he ſaw, as follows:

A Letter from Mr. Blair to Mr. Oliphant.

"Sir,

"As you have done me the honour to give me a ſight of Mrs. Craib's caſe, I cannot reaſonably decline writing (as you deſire) a ſhort ſtatement of what I then witneſſed.

"When I formerly ſaw the patient with Dr. Niſbet, appearances ſeemed very much in her favour, and there was even room to hope for a complete cure; but, yeſterday, I obſerved the glandular enlargement and induration of the right breaſt had increaſed, the ſkin and integuments of the left ſide were irregular, tuberculated, and adhering to the pectoral muſcle; beſides which there were various hardened ſubcutaneous glands about the left breaſt, extending towards the armpit.

"I here beg leave to conclude my evidence. If it will ſerve to throw light on the truth, you are welcome to make uſe of this letter. I remain,

"Sir,

Great Ruſſel Street, Bloomſbury, "Your obedient humble ſervant,
January 9, 1801. "William Blair."

The following two caſes, which I am afraid will be followed by ſeveral others, will further afford a reaſonable juſtification for my giving the fair ſtatement of Mrs. Craib's caſe.

In February laſt Charlotte Logan, at Mr. Campbell's, Newcaſtle Street, Strand, aged 46, had her menſtrua ſuddenly ſuppreſſed apparently by a violent emotion of the mind; and in the ſummer perceived a ſmall ſcirrhus in her left breaſt, which became painful in Auguſt following.

About September 25th ſhe was attacked with fever, ſaid by the gentleman who attended her, to be independent of the condition of the breaſt; but on the incumbent ſkin of it, and to a great extent on the left ſide, a diffuſed redneſs appeared, attended with much pain, which was removed by repeated bleedings with leeches, and left the ſcirrhus in the ſame condition as before.

From

From the alarming ſtate of the breaſt, ſhe was adviſed to go into St. Bartholomew's Hoſpital, wherein ſhe remained nearly a fortnight. After a conſultation of the ſurgeons, exciſion of the diſeaſed part was concluded on: Hearing, however, of Dr. Niſbet's cure of Mrs. Craib in the beginning of October, her friends removed her ſuddenly to be under his care, who pronounced " he would cure her, at fartheſt, in ſix weeks."

On the ſeventh week, it was ſaid that the breaſt would ſuppurate and burſt in eight days. But a few more days were paſſed; and ſuppuration did not follow.

After being nine weeks under his frictions, ſhe was taken with a violent pain in her head, when ſhe was deſired to keep her bed. For a fortnight ſhe was confined: The Doctor only ſaw her once, and never after returned, for reaſons beſt known to himſelf, having attended her eleven weeks. There was great increaſe of the pain and diſeaſe in the breaſt, and an oozing from around the nipple, forming troubleſome incruſtations.

A week after this ſhe applied to me, and I found the whole left breaſt ſcirrhous, ſeveral cuticular glands inflamed, which appeared firſt under the Doctor's care, and an incruſtation round the nipple; the whole attended with moſt excruciating pain, and vaſt agitation of mind. She now wanted me to pronounce whether it could poſſibly be cured: Not being bold enough to ſatisfy her wiſhes, ſhe was put upon a plan that was conſidered as too expenſive in the purchaſe of malt.—Though the application ſoothed the breaſt, yet I did not ſee her after, till I met her on the 1ſt inſtant at Mrs. Craib's for the ſecond time, and found ſhe had an ointment ſpread on paper: I therefore conclude ſhe is now under the direction of ſome old woman. During this interval ſeveral lymphatic glands behind the clavicle, and up the ſide of her neck, have become enlarged.

There is alſo the caſe of Mrs. ———, who put herſelf under the care of Dr. Niſbet, in conſequence of the firſt publication of Mrs. Craib's caſe; but the reſult of that can be " beſt related by the undertaker."

I have now, Gentlemen, laid before you and the public all the facts deſerving their notice reſpecting Mrs. Craib's caſe. It was very much hoped for, that perſonality might have been avoided: However, unfortunately a ſpirit has ſhewn itſelf, living and thriving on controverſy, that has dragged me into meaſures I highly diſlike, and I muſt, in future, leave to itſelf, a ſpirit which wiſhes to ſubſtitute ſophiſtry for plain facts, diſhonourable inſinuation for claim of rectitude, and ſcholaſtic reaſoning for practical opinion. I am, &c.

Percy Street, Jan. 8, 1801. ISAAC OLIPHANT.

MONTHLY REPORT of DISEASES,

Admitted under the Care of the PHYSICIANS of the FINSBURY DISPENSARY, St. John's Square, Clerkenwell.
From Dec. 20, 1800, to Jan. 20, 1801.

	No. of Cases.		No. of Cases.
Continued Fever - -	69	Menorrhagia - - - - -	7
Small-Pox - - - - - -	3	Melancholia - - - - -	1
Eryſipelas - - - - - -	5	Dyſpepſia - - - - - -	16
Cynanche Tonſillarum -	7	Hyſteria - - - - - -	3
Phthiſis Pulmonalis - -	11	Paralyſis - - - - - -	4
Hæmoptyſis - - - -	4	Gaſtrodynia and Enterodynia	6
Pneumonia - - - - -	2	Epilepſia - - - - - -	4
Cough and Dyſpnœa - -	57	Cephalæa - - - - - -	7
Catarrh - - - - - - -	4	Dropſy - - - - - - -	11
Dyſentery - - - - - -	2	Aſthenia - - - - - -	19
Diarrhœa - - - - - -	6	Infantile Diſeaſes - - -	13
Chloroſis and Amenorrhœa	18	Chronic Eruptions - -	11

The caſes of typhus will appear to have very conſiderably increaſed in number during the laſt month; but they have not, in general, been characteriſed by that violence of frenzy which was ſo eſpecially remarkable in the fevers of the ſummer and autumn. The extraordinary determination to the head that prevailed during thoſe ſeaſons, in various inſtances, produced not merely the common delirium of fever, but an abſolute and a ſpeedily fatal inſanity.

The preſent typhus is more particularly marked by a profound coma, and a general proſtration of the ſtrength.

In ſeveral caſes that at firſt ſeemed to be deſperate, a recovery, it might almoſt be called a reſurrection, was brought about principally by very ſtrong ſtimuli, applied in ſmall but frequently repeated doſes. After the inſide had been *rinced* by emetics and aperients, brandy with wine was often adminiſtered in as large a quantity as the patient was ſuppoſed able to ſwallow, without the inconvenience of ſucceeding ſickneſs or intoxication.

This mode of treatment, when it was aſſiſted by the daily waſhing of the whole body with cold water, the free admiſſion of freſh air into the ſick room, the frequent change of linen and bed clothes, moderate doſes of opium at night, and the careful abſtraction of every thing that might tend to interrupt the ſleep of the patient, or to diſturb the tranquillity of his mind, has ſeldom failed to induce a ſpeedy convaleſcence, the progreſs of which towards an entire reſtoration of health and ſtrength was afterwards accelerated by the application of nouriſhing diet, and a variety of medicinal corroborants.

It ſhould, however, be underſtood, that the directions of the phyſician, thoſe eſpecially that related to the adminiſtration of wine and food, could not, in many caſes, be carried into effect, in conſequence of the poverty of the patient, and the ſlow and ſcanty aſſiſtance of parochial charity.

Too generally is it imagined that work-houſes are not provided only, but alſo are fully adequate to the relief of thoſe who labour under the extremity of want. But if any credit may be paid to the teſtimonies of many of the poor creatures that crawl to ſupplicate the medical relief of the Diſpenſary, this is far from being, in every inſtance, actually the caſe.

Not longer ſince than yeſterday, to the perſon who is now employed in writing this Report, a boy was brought, whoſe attenuated and nearly lifeleſs form awakened a ſentiment, and even an exclamation of compaſſion from all the ſurrounding patients.

According to the mother's account, whoſe appearance and manner gave not the ſlighteſt pretence for ſuſpecting the veracity of her aſſertions, her child had been for ſome time depoſited in a work-houſe, but had there been deprived of what was abſolutely eſſential not to his health merely but almoſt to the bare ſupport of his exiſtence.

By ſome the Reporter has been charged with indiſcretion, in thus unveiling the extreme wretchedneſs of the poor. But, deaf to the dictates of worldly prudence, and inſenſible to the impulſes of ſelfiſh intereſt, never, by ſuch motives, will he be tempted to decline the declaration of facts which he knows to be true, and the diſcovery of which he cannot but regard as of unſpeakable importance.*

One of the caſes of melancholia, recorded in the liſt of diſeaſes, is deſerving of attention, from the circumſtance of the patient's not having been afflicted with it until after the deprivation of his ſight. The reflection upon that loſs could not fail for a time to have been itſelf a courſe of uneaſy feelings; but the continuance of his depreſſion might perhaps be better accounted for by his not being any longer able, in conſequence of this loſs, to purſue his uſual active employment, by its withdrawing from him the natural and exhilarating ſtimulus of light, and by its precluding altogether the poſſibility of that amuſement and diverſion of mind, which in general is ſo conſtantly derived from the contemplation of external objects. By confining the ſenſibility within a ſmaller compaſs, it condenſed and increaſed its force.

A caſe occurred which the phyſician who was conſulted with regard to it, ſuſpected to be an aneuriſm of the aorta. But from a degree of diffidence with regard to the accuracy of his concluſion, he had recourſe, for a removal of his doubts, to the advice of a reſpectable ſurgeon†, who, after an attentive examination, having formed

* Since the publication of the laſt Report, a donation of twenty pounds has been preſented, by a perſon well known for his humanity toward the poor, to the phyſicians of the Finſbury, and ſeveral other Diſpenſaries in London, for the relief of thoſe of their neceſſitous patients who are confined to a ſick bed; and, to uſe the words of the gentleman through whoſe medium it was tranſmitted, " to be diſtributed in ſmall ſums proportioned to *the number of ſtarving children*."

† If the name of the gentleman alluded to was to be mentioned, *it would not only* give an additional confirmation to the fact, but alſo would demonſtrate to the public the extreme uncommonneſs of a caſe that could have been miſtaken by a perſon of his profeſſional learning and ſagacity.

formed more decidedly the ſame opinion, ſcrupled not inſtantly to pronounce the ſentence of ſpeedy death upon the unfortunate patient.

Since that time, however, there has occurred ſtrong reaſon to believe that the complaint was merely a nervous affection. It appears to have originated from diſtreſs of mind; it was induced often, and always was aggravated by any thing that agitated her ſpirits; it was accompanied by Borboriemi; all theſe ſymptoms uniformly increaſed or diminiſhed together: For about ten weeks they have all been gradually diſappearing; and for this laſt fortnight have ſcarcely been felt at all, in conſequence, as it is reaſonable to ſuppoſe, of the remembrance of the painful circumſtances from which her diſorder had at firſt originated, having, during that time, become more feeble; and alſo perhaps from a favourable change that, according to her own account, had lately taken place in the ſtate of her external circumſtances.

In many diſeaſes, eſpecially in thoſe that are called nervous, a ſuſpenſion of the ſymptoms has not unfrequently been found to occur in conſequence of the ſame, or indeed any other complaint attacking one who is particularly near and intereſting to the patient. Even in caſes of fever, a mother ſeldom finds herſelf ſick until the ſickneſs of her child is over. The attention and anxiety which are conſtantly kept up, do not prevent the body from receiving contagion, but often prevent the mind from being for ſome time aware of its influence. No ſooner, however, does the recovery of the child take place, than, in general, it is ſucceeded by the indiſpoſition of the parent; which, although it has been delayed, cannot fail to be aggravated and rendered ſtill more dangerous by the extreme degree of labour, watchfulneſs, and ſolicitude that had immediately preceded its attack.

In the remarks which, in a former part of this report, have been made with regard to the uſe of ſtimuli in caſes of deſperate diſeaſe, it was far from the intention of the writer to encourage a recourſe to them in a ſtate of health and vigour. The application of any extraordinary ſtimulus to the human ſyſtem muſt invariably abridge the period of its poſſible duration; but at the moment when it is about to be extinguiſhed, it is neceſſary to *blow* the flame of life, although by that means you conſume a part of the fuel that is neceſſary to its ſupport.

The grand object to be attended to in what is vulgarly denominated a low fever, is by artificial means to give, for a time, a degree of ſtrength that may ſupport the patient during *the period of his ſtruggle with death.*

The ſtimulating mode of practice ſeems to be particularly ſuited to the conſtitution of thoſe who live under the influence of the ſedative atmoſphere of the metropolis. In London it is ſcarcely poſſible for a man to receive into his lungs a draught of air that has not been in ſome other perſon's lungs before. This *second hand* air is not only always injurious to health, but ſometimes proves almoſt immediately deſtructive of exiſtence.

It is a circumſtance well worthy of remark, that out of the multitude of caſes that within the extent of the Finſbury diſtrict have, for this laſt year, been attacked by typhoid contagion, few, comparatively, have fallen victims to it, but thoſe whoſe previous habits of debauchery or intemperance had reduced them into a ſtate that was peculiarly ſuſceptible of diſeaſe, and almoſt altogether inſenſible to the operation of medicine. Bark has but a feeble effect upon a ſtomach that has been long accuſtomed to brandy; and a frame, the ſtamina of which have been deſtroyed by an inordinate indulgence of the ſenſual appetites will, in no inſtance, be regenerated, or even find conſiderable relief, from a recourſe to any of the pharmaceutical preparations*.

Red-lion Square. J. R.†

Account of Diſeaſes in an Eaſtern Diſtrict of London, from the 20th of December, 1800, *to the 20th of January,* 1801.

	No. of Caſes.		No. of Caſes.
ACUTE DISEASES.		Colica	2
Continued Fever	18	Enterodynia	5
Pneumonia	2	Hyſteria	7
Sore Throat	3	Vertigo	4
Acute Rheumatiſm	2	Hemiplegia	2
Dyſentery	2	Cephalæa	8
CHRONIC DISEASES.		Amenorrhœa	9
Cough	15	Fluor Albus	7
Cough with Dyſpnœa	19	Hæmorrhois	3
Hydrothorax	4	Dyſuria	2
Catarrh	5	Chronic Eruptions	12
Hæmoptyſis	4	Lumbago	2
Gaſtrodynia	10	Sciatica	2
Aſcites	3	Chronic Rheumatiſm	19
Anaſarca	5		

PUERPERAL

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* The idea which it is here meant to inculcate, and indeed has been more than once inculcated in theſe reports, has ſeldom been enough attended to by patients, or acted upon in the practice of phyſicians. Upon the minds of the former its truth cannot too deeply be impreſſed, although from the future profits of the latter a conviction of its truth might chance to deduct no ſmall portion of that pecuniary emolument which they have hitherto been in the habit of extracting from the mental, more perhaps than from the corporeal imbecility of their fellow creatures.

† Dr. Webb having retired from his official connection with the Finſbury Diſpenſary, theſe reports cannot any more be ſanctioned by the initials of a name that long will be remembered; and as long as it is remembered, will be reſpected by the ſupporters, and ever will be dear to the memory of the patients of that charitable inſtitution. From the friendſhip and ſociety of his late amiable colleague, the writer of this article has been indebted for relief under the preſſure of almoſt unprecedented calamity; and from his kindneſs and profeſſional ſkill has derived important aſſiſtance in diſcharging the duties annexed to an anxious and moſt laborious ſituation.

PUERPERAL DISEASES.		INFANTILE DISEASES.	
Menorrhagia Lochialis	5	Aphthæ	4
Low Puerperal Fever	4	Convulsio	3
Rhagas Papillæ	3	Diarrhœa	8
		Dentition	2

We are sorry to announce the continuance of a disease, which we have in several former Reports, either particularly described, or incidentally referred to, as the reigning epidemic: A continued fever of the typhoid character, and attended with unusually severe affections of the head, still prevails very generally.

The delirium which commonly attends fevers of this class, is rather a confusion of ideas and an interrupted recollection, attended with low mutterings and expressions of anxiety, than any discovery of rage and violence; but in many of the cases now referred to, there has been an active and furious delirium, which has rendered coercive means necessary to keep the patient in bed, or prevent him from doing mischief. The disease has in some instances approached in a very insidious manner, and before the state of the pulse and other symptoms have created any alarm, either coma or delirium have occurred, and excited apprehensions for the safety of the patient; and in too many instances the disease has proved fatal.

In a late report, we had occasion to remark that the Small-pox proved uncommonly fatal; and by a comparison of the lists now referred to, it will appear that the number of deaths by this disease during the last year, is in the proportion of two to one of the preceding year. The number of deaths by Fever, in the same period, has also increased by more than one-third. *

* *The General Bill, containing the Diseases and Casualties of the year 1800, is unavoidably postponed till our next.*

MISCELLANEOUS INTELLIGENCE.

CIT. VAQUELIN has discovered in *the common house leek (sempervivum tectorum L.)* a great quantity of malat of lime (malate de chaux) even in a greater proportion than it is met with in any fruit. It may be extracted in two different ways; the one by evaporating the juice, and pouring alkohol on the residuum to free it from the saccharine parts. After being sufficiently washed, it is treated with half its weight of sulphuric acid, mixed with seven or eight parts of water But as by this method, a portion of sulphat of lime is left in the malic acid, the following seems to be preferable. A solution of acetite of lead is poured upon the juice of the sempervivum; and after having edulcorated the precipitate, it is decomposed by diluted sulphuric acid. In the same way the purest malic acid

acid may be obtained, which is but very little impregnated with colouring particles. The ſame plant contains likewiſe a conſiderable portion of ſaccharine matter, and affords alſo a ſort of brandy: And after having ſeparated the malat of lime, a ſyrup may be got by evaporation, much reſembling that of apples. The root gives alſo a great portion of malat of lime. This is the only plant in which it has been found. Hitherto it has been only extracted from ſome fruits, nuts, berries, &c. It is very probable that malic acid is contained in the ſame ſtate in all ſucculent plants, and particularly in the genus of *Sedum*. (*Annales de Chemie*, 101.)

According to the reſearches of the ſame chemiſt, *the vitrum antimonii*, or glaſs of antimony, contains in 100 parts, 12 parts, or more frequently 9 or 10 parts of *ſiliceous earth*. It is probable, that it originates from the crucibles in which the oxyd of antimony is melted on the ſiliceous earth, on which this oxyd has, like that of lead, a great action. It is eaſily conceived, that its proportion muſt vary according to the degree of heat that is employed, and the time it is kept melting. It is likewiſe poſſible, that this earth comes from the furnace in which the ſulphur of antimony is calcined. This circumſtance, undoubtedly, accounts for the inconſtant effects of emetic tartar, which is always produced, if the ſiliceous earth is not ſeparated by repeated cryſtallizations, or if the ſolution is merely evaporated, a practice ſtill very common amongſt apothecaries. Many perſons will probably wonder how ſiliceous earth ſhould diſſolve in tartaric acid, as it reſiſts the action of acids much ſtronger than this; but it may be remarked, that as ſoon as it is united with any alkali, earth, or oxyd of metal, a weak acid is capable of diſſolving it.

It is therefore adviſable in preparing emetic tartar, to diſſolve as much of the vitrum antimonii in cremor tartari as is poſſible, to filter the ſolution warm, and let it afterwards evaporate till the reſiduum becomes dry. This is again diſſolved in hot water, and left to cryſtallize. By this means all ſiliceous earth diſſolved in the acid ſeparates at the end of the evaporation, and diſſolves no more; and at the ſame time the emetic is obtained in a greater quantity and a purer ſtate, than when the earth oppoſes the cryſtallization. (*Ibid.*)

Mr. Hermbstaedt mentions in a letter to Profeſſor Scherer of Halle, that he is preparing a new edition of his Experimental Chemiſtry, in which he conſiders the calorique or calorific principle, not as warm itſelf, but only as producing heat and the ſenſation of warmth merely as a phenomenon of extenſion, which affects our nerves, as warmth; on this account he calls the calorique, *thermogenium*. He likewiſe conſiders the light as lighting or illuminating, when it affects our eyes, and names it light-producing principle, *Photogenium*. To the unknown acidifiable baſis of Borax, he aſſigns the name of *Boraceum*; that of the fluoric acid he diſtinguiſhes with the name of *Fluoreum*; that of muriatic acid with

Muriaeum. He differs from Richter by confidering the light-producing principle as fimple and not yet decompofed; but he allows, at the fame time, that it is contained in all incombuftible fubftances. (*Scherer's Journal of Chemiftry,* in German, *Vol. IV. No.* 22, 1800.)

We are informed, that Mr. Blair, of the Lock Hofpital and Finfbury Difpenfary, affifted by feveral other refpectable furgeons, has been fome time engaged in writing a comprehenfive *Syftem of Medical and Operative Surgery,* adapted to the prefent improved practice at the London Hofpitals, &c. We fhall take an early opportunity of laying before our readers a more particular account of this undertaking, as a Profpectus is faid to be in the prefs.

Mr. William Henry, of Manchefter, has in the prefs, and in confiderable forwardnefs, a fmall work, intended, partly, to facilitate the acquirement of chemical knowledge to perfons entering on this ftudy without the benefit of an inftructor; and, partly, as a portable companion for the ufe of more advanced ftudents. The firft part will contain directions refpecting the mode of learning chemiftry, and, alfo, an arranged feries of experiments, neceffary to be performed by thofe who intend to become acquainted, by actual obfervation, with the chemical properties and habitudes of bodies. More minute and precife directions will be given for conducting thefe experiments with fuccefs, than are to be found in other elementary books. The fecond part will comprife, Summary Inftructions for the Analyfis of Mineral Waters and of Mineral Bodies in general. And the third part will point out fome of the ufeful applications of chemical agents in detecting adulterations, difcovering poifons, &c. The work will form one fmall pocket volume; and it may be proper to remark, that it will not at all interfere with the excellent little Manual lately publifhed by Mr. Parkinfon, the plan and objects of which are perfectly different.

Dr. Ferguson, of Aberdeen, has in the prefs an Effay on Fever, which will be publifhed in a few months.

Dr. Pearson's Lectures on the Practice of Phyfic, Materia Medica, and Chemiftry, will re-commence in the Elaboratory, Whitcomb Street, Leicefter Square, on Tuefday, February the 3d, at eight o'clock in the morning.

TO CORRESPONDENTS.

The Editors are again obliged to apologize to their Correspondents for the omission of several valuable Communications, and particularly for delaying an account of many Medical Publications, both domeftic and foreign.